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Building Trust in Rural Producer Organizations in Senegal: Results from a Randomized Controlled Trial

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ABSTRACT

Building Trust in Rural Producer Organizations in Senegal: Results from a Randomized Controlled Trial*

Trust is crucial for successful collective action. A prime example is collective commercialization of agricultural produce through producer organizations. We conduct a cluster-randomized controlled trial in rural Senegal in which we vary the number and the type of smallholder farmers – members and/or leaders of local producer organizations – invited to a three-day training on collective commercialization. We use this variation to identify effects on intra-group trust, both direct treatment effects of having participated in the training and spillover effects on farmers who did not partake. Looking at different measures of trust in leaders' competence and motives and of trust in members we find that participating in the training significantly enhances both trust in leaders and trust in members. For trust in leaders, we also find a strong spillover effect. Our findings suggest that relatively soft and non-costly interventions such as a group training appear to be able to positively affect trust within producer organizations.

JEL Classification: D71, O12, Q13

Keywords: rural producer organizations, smallholder farmers, trust, Senegal

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1 Introduction

Rural Producer Organizations (RPOs)¹ can provide smallholders in developing countries with better access to input and output markets (Markelova et al., 2009; Rondot and Collion, 2001; The World Bank, 2008). Acting collectively within RPOs is often seen as an effective means to reduce transactions costs, to gain bargaining power, to obtain necessary market information, and to get access to new technologies and high value markets (Devaux et al., 2009; Poulton et al., 2010; Stockbridge et al., 2003). While examples of successful collective action exist, such as coffee production, grading and export in Ethiopia (Kodama, 2007), green bean exports in Kenya (Narrod et al., 2009) and cotton production and exports in Mali (Tefft, 2004), many RPOs in developing countries struggle to offer the type of services that would lead to higher output prices for their members (Bernard et al., 2008a, 2010; Fafchamps and Hill, 2005; Markelova et al., 2009). Instead of commercializing their produce collectively, smallholders frequently opt for selling it individually to traders at the farmgate (Fafchamps and Hill, 2005).²

Whether collective commercialization takes place or not is highly correlated with the ability to coordinate and the prevalence of trust³ within a RPO (Hill et al., 2014; Shiferaw et al., 2011). Mistrust between farmers can even lead to the disintegration of RPOs (Masakure and Henson, 2005). Typically, RPOs are member-driven organizations whose success depends on their members' participation in joint activities. Reasons for members being reluctant to participate in collective commercialization largely play out at the intra-RPO level (Bernard and Wouterse, 2015). First, farmers may struggle with liquidity constraints and hence

¹“Producer organizations are membership-based organizations or federations of organizations with elected leaders accountable to their constituents. They take on various legal forms, such as cooperatives, associations, and societies” (The World Bank, 2008, p. 154). In this paper, we analyze farmer groups in Senegal that provide members with services. Hence, they differ from NGOs which also provide services to non-members (Bernard and Wouterse, 2015).

²This paper implicitly builds on the assumption that collective commercialization is more efficient than individual marketing. See, for example, Bernard et al. (2008b) for evidence that cooperatives' members achieve higher prices for their produce than non-members in Ethiopia, Shiferaw et al. (2008) for evidence that collective commercialization achieves higher output prices than individual sales to traditional buyers in Kenya, and Wollni and Zellner (2007) for evidence that commercialization through cooperatives achieves higher output prices in Costa Rica. See Section 2.2 for details about prices achieved by farmers in our sample.

³We define trust as “a particular level of the subjective probability with which an agent assesses that another agent or group of agents will perform a particular action” (Gambetta, 2003, p. 277) and hence do not regard it as being a form of social capital in itself, but rather as being a measurable link between social capital and collective action (Ostrom and Ahn, 2003).

decide against participating in collective commercialization as it involves substantial delays in payment as compared to individual farmgate sales (Fafchamps and Hill, 2005). Second, farmers need to have sufficient trust in their leaders' motives and competences. The sales processes of collective commercialization are typically executed by few leaders and are not transparent for members. Members may fear to not be paid at all or that price increases that occur through bulking will not be shared with them fairly (Buck and Alwang, 2011; Hill et al., 2014). Third, a farmer's decision to participate in collective commercialization is likely to be affected by her belief about the behavior of fellow farmers. Trust in fellow members' and their commitment is needed to believe that the minimum number of produce will be reached for bulk sales and, hence, to decide to participate (Bernard et al., 2014).

According to the theoretical framework provided by Ostrom and Ahn (2003), success and failure of collective action is determined by a complex configuration of various forms of social capital—trustworthiness⁴, networks and institutions⁵—which enhance trust. Trust is seen as the core link between social capital and collective action. It reduces opportunistic behavior and transaction costs (Collier, 2002; Ostrom and Ahn, 2003; Putnam, 1993). “Trust lubricates cooperation” (Pretty and Ward, 2001, p.211). We expect that a training intervention can strengthen existing horizontal and vertical networks and may even create new ones through repeated interaction of farmers. We further expect a training intervention to clarify the process of collective commercialization and the rules involved and, thus, to create a common understanding of the relevant institutions. We hypothesize that such increases in social capital will lead to a positive effect of a training intervention on trust in members and on trust in leaders.

We conduct a cluster-randomized controlled trial to test these hypotheses in the context of RPOs in Senegal. We randomly invited members and/or leaders to participate in a three-day training to build social capital in order to induce members and leaders to coordinate towards successful collective commercialization. The training itself consisted of three days of interactive discussion on the benefits, conduct and constraints of collective commercialization and was executed in

⁴The term trustworthiness refers to intrinsic motivation as an independent reason for behaving cooperatively (Ostrom and Ahn, 2003).

⁵Institutions are the rules of the game that people devise (North, 1990). They specify which actions and outcomes are required, prohibited, or permitted, and which sanctions are authorized (Ostrom and Ahn, 2003).

collaboration with GRET (a French NGO) and PINORD (a Senegalese NGO). In all treated RPOs, a general assembly was convened shortly after the training to discuss its content with non-participants. All participating RPOs had stated collective commercialization as one of their main objectives despite facing important difficulties in aggregating their members' produce.⁶

We assess how this intervention affects trust in a sample of 798 smallholder farmers from 73 RPOs. We analyze both intention-to-treat effects, i.e., the treatment effect of the invitation to participate in the training, and treatment effects of the training itself. Our main outcome variables of interest are survey measures of trust in leaders' competence and motives and survey measures of trust in members (Twyman et al., 2008). We distinguish between trust in leaders' integrity, informational advantage, efficiency and negotiation skills, and general and farming-specific trust in members.⁷ In each RPO we randomly varied the number of members and leaders invited to the training and interviewed both invited and non-invited individuals. This enables us to analyze (i) the average treatment effect of whether or not a RPO was selected for training, (ii) the direct treatment effect of whether or not an individual was selected for training, and (iii) the spillover effect on non-treated individuals in treated RPOs.

Our results suggest a positive treatment effect of the training on measures of both trust in leaders and trust in members. For trust in leaders, the effect is especially pronounced for trust in their integrity and their informational advantage. Moreover, results suggest a strong spillover effect on trust in leaders of non-treated members, i.e., that the sizable positive average treatment effect is not to be attributed solely to personal treatment. The spillover effect is especially pronounced for trust in leaders' integrity and their negotiation skills. We show that while RPOs may face several constraints with respect to coordination towards collective commercialization, a relatively simple and non-costly group training may contribute to a substantial change in trust in leaders and trust in members and, thus, to clearing a barrier to collective commercialization and other collective endeavors. Social capital is often thought to increase with use, thus, the training may well have been the starting point for more collective endeavors to come.⁸

⁶See Section 3 for details on the training intervention and the sample.

⁷See Section 4.2 for details on the measures of trust.

⁸Bernard et al. (2014) show for a subsample of groundnut farmers that the training intervention indeed had a positive effect on the quantity of produce commercialized collectively through the RPOs.

The remainder of this paper is organized as follows: We begin, in Section 2, by describing the institutional background of RPOs in Senegal and the issues with collective commercialization through such organizations. In Section 3, we describe the cluster-randomized controlled trial. In Section 4, we develop the hypotheses tested in this paper and describe our measures of trust. In Section 5, we present our empirical strategy and estimates of the treatment effects on RPO-level and, in Section 6, on individual level. We conclude in Section 7.

2 Background: RPOs in Senegal

2.1 Institutional setting

Seven out of ten rural households in Senegal are members of RPOs, and RPOs have expanded rapidly in number and membership in the last decades. Between 1982 and 2002, the percentage of villages with RPOs rose from 8 to 65 percent in Senegal (The World Bank, 2008). Senegal is a country with a vast array of rural institutions in thousands of villages and with strong national-level organizations (Bernard and Wouterse, 2015).

The history of agricultural and development policy in Senegal since independence can be divided into two main periods: two decades of state intervention followed by two decades of liberalization. Like in many other developing countries, the Senegalese government initiated cooperatives in the 1960s which primarily dealt with the commercialization of groundnuts. The entire value chain of agricultural production of groundnuts and cotton was organized and controlled by state entities such as producer cooperatives that were in charge of providing extension, inputs and credit to farmers as well as of collecting, processing and exporting output. Farmers typically considered cooperatives as an extended arm of the government rather than as an organization that they owned. Support of the international community led to the creation and development of regional organizations for rural development which aimed at specializing and transforming agricultural systems in the different agro-ecological zones. However, these organizations focused on the cultivation of a particular type of crop which was in contrast to the reality of rural smallholders typically cultivating several types of crops (Bernard et al., 2014; Bernard and Wouterse, 2015).

In the 1970s, this system of rural development went into crisis. Throughout

the country, farmers founded the first independent farmers' organizations to represent their own interests. In 1978, the Federation of Senegalese NGOs (FONGS) was established to reinforce solidarity and communication among farmer groups, to foresee their training needs and to interact with the outside world on their behalf. Foreign stakeholders became increasingly interested in farmers' organizations as partners for development projects. In the 1980s, the new agricultural policy which followed the near bankruptcy of the country and a bail-out deal by the International Monetary Fund advocated the complete withdrawal of the state from agriculture. Consequently, the government facilitated the emergence of a new category of organizations with a more flexible legal status. These groups grew rapidly due to the support of their creation and their preferential treatment for obtaining credit. To defend its own interests, FONGS undertook ambitious projects at the national level such as cereal banks. In 1993, FONGS initiated the creation of a platform (CNCR) consisting of seven national federations of farmers, livestock producers, fishermen, horticultural producers and rural women. Its objective was to promote dialogue and the exchange of experience between its members, to encourage pooling of resources and competences and to serve as voice for the farmer movement versus the state and donors. In 1995, two other federations joined bringing the platform's total number of members up to about 3 million rural dwellers. In 1996, an NGO (ASPRODEB) was founded that could disburse funds of various donors and offer services to rural producers (Bernard et al., 2014; Bernard and Wouterse, 2015; Mercoiret, nd; Réseau des Organisations Paysannes et Pastorales du Sénégal, 2008; The World Bank, 2008).

The role, functioning and capacity of RPOs has changed significantly with the gradual liberalization of the agricultural sector. They have evolved towards independent member-controlled entities capable of dealing with various kinds of partners. Yet, these RPOs face new challenges in ensuring the commitment of members. This is particularly true for activities related to collective commercialization of agricultural output. In fact, while RPOs remain active in input and credit provision, their capacity to aggregate output has been weakened considerably. At time of harvest, producers are now visited by private collectors and traders. This has led to important side-selling, produce is not marketed solely through the RPOs. As a result, RPOs are seldom able to aggregate the necessary amount of produce to trigger economies of scale and bargaining power at time of commercialization. This is despite isolated evidence that RPOs in Senegal can

in fact provide their members with profitable and reliable output commercialization services. In sum, in the absence of a sufficient amount of aggregated output, RPOs are not in a position to effectively obtain higher output prices which leads to further side-selling and a lack of interest of members for commercialization services offered by these organizations (Bernard et al., 2014).

2.2 Collective action in Senegalese RPOs

Generally, problems of collective action arise “whenever individuals face alternative courses of actions between short-term self-regarding choices and one that, if followed by a large enough number of individuals in a group, benefits all” (Ostrom and Ahn, 2003, p. xiv). In the context of commercialization of agricultural produce, farmers who are organized in RPOs can choose between selling their produce directly at the farmgate to a local trader with immediate payment and selling it collectively through their RPO. Farmers in our sample cultivate either groundnuts, rice, or onions as their main crop.⁹ While groundnut farmers primarily sell their groundnuts either individually or collectively for private gains, onion farmers have been observed to engage in joint collection and commercialization of firewood. Rice farmers use part of their gains from selling their rice (privately or collectively) for financing irrigation systems.

For collective commercialization of any type of produce, the RPO’s leaders will typically negotiate with a contract buyer who offers a higher price than the local traders if a minimum amount of produce is sold. This quantity is typically too large to be reached by just one individual farmer such that complying with the contract requires coordination among the members of the RPO. Moreover, these contracts tend to be rather discrete in the sense that they are void if a certain threshold of aggregated produce is not met and payments are usually made some time after the farmers delivered their produce to the RPO for collective commercialization (Bernard et al., 2014). In our sample, two thirds of the farmers who report to have sold to traders in the 2010/2011 agricultural season (March 2010 to March 2011) state that they received the payment on the day of the transaction while only one quarter of the farmers who sold through the RPO report to have received the payment on the day the transaction took place.

Senegalese RPOs appear to be facing significant constraints when it comes

⁹There is no heterogeneity within any RPO in our sample regarding the main crops cultivated by members and leaders.

to collective action in the form of collective commercialization of agricultural produce. Although RPOs in our sample were selected based on collective commercialization being one of their main objectives, only roughly one third of the RPOs in our sample report to have engaged in collective commercialization in the 2010/2011 agricultural season.¹⁰ On average, farmers who sold their produce through the RPO in the 2010/2011 season achieved higher prices per kilogram than those who sold their produce individually. Only one quarter of the farmers who sold their produce individually to local traders regularly dealt with the same traders. When asked about difficulties faced when selling their produce individually, only 20 percent of the respondents indicated that there were no major problems. For the remaining respondents, the main issue was lack of transportation means to reach more lucrative markets (cited by 48 percent) followed by insufficient visits by potential buyers (17 percent), lack of knowledge about current prices (15 percent) and lack of sufficient quantity of produce to be able to negotiate a better price (8 percent).

While lack of financial means and technical capacities at the RPO level are likely to be significant constraints to collective commercialization, members' perception of collective commercialization and issues of coordination between members also seem to represent important constraints, independent of the type of crop to be sold collectively. Leadership has been shown to be a key element of successful coordination within RPOs (Agrawal, 2001; Bernard and Wouterse, 2015; Markelova et al., 2009).¹¹ On the one hand, leaders need to have the necessary skills for the collective marketing and relevant links to outsiders. On the other hand, members need to trust their leaders (Arcand, 2002; Markelova et al., 2009). In those RPOs that engaged in collective commercialization, only 44 percent of the farmers delivered part of their produce to the RPO to be sold collectively. Respondents state three main reasons for why they chose to sell individually in spot market-like transactions to local traders rather than collectively through their RPO: most find collective commercialization to be too risky (without specify-

¹⁰We consider a RPO to be engaged in collective commercialization if at least one farmer sold his or her main produce (groundnut, onion, or rice) through the RPO.

¹¹A commonly found organizational structure in Senegalese RPOs involves a relatively circumscribed sub-group of members bearing more responsibilities and more influence than others. Leadership status is often associated with traditional social roles (Arcand, 2002).

ing the exact nature of this risk),¹² some do not believe that it offers them a better price than selling individually, and to others collective commercialization appears to be a complicated process that takes up too much time.¹³

RPOs engaging in collective commercialization differ significantly from those who did not engage in collective sales in the 2010/2011 agricultural season (see Table 1).¹⁴ Their members are younger, own more motorcycles, are more likely to have savings accounts with banks and MFIs, and have larger fields. They cultivate groundnuts and rice more often than onions. Moreover, we find significantly higher average trust levels in RPOs that engaged in collective commercialization than in those that did not.

Farmers who contributed part of their produce to collective commercialization in the season prior to the baseline data collection appear to be significantly more trusting than those who did not contribute (see Table 2). Moreover, they are more likely to be literate than those who did not contribute, less likely to be a member, less likely to be related to a leader, more likely to have a savings account, have larger fields and more motorcycles, and are younger. They are also more likely to come from ethnically heterogeneous RPOs, have been members of their RPO for a shorter time period, are more likely to be cultivating groundnuts and less likely to be cultivating onions. Looking at different measures for trust, we find a significantly positive pairwise correlation between several trust measures and an indicator for whether a RPO engaged in collective commercialization. Similarly, we find a significantly positive pairwise correlation between several trust measures and individual participation in collective commercialization (see Table 3).

¹²The risk inherent in collective commercialization may be categorized into two types of risk: the price risk associated with freely working markets and the risk associated with being dependent on others' behavior. On the intra-RPO level, the outcome of collective commercialization depends on the number of fellow farmers who participate as well as the competence and behavior of the leaders involved in the transactions.

¹³50 percent of the respondents stated risk as a reason, 30 percent the price and 26 percent the inconvenience (numbers do not add up to 100 percent as answers were given in free text and each person could provide several reasons). Information on this question is only available for a subsample of groundnut farmers.

¹⁴See Appendix A for a definition of all variables used in the empirical analysis.

Table 1: Descriptive statistics by collective commercialization activity on RPO level (RPO characteristics and RPO averages of individual characteristics)

Variable	Mean (N=69)	Standard deviation	Mean if RPO not active in collective commercialization (N=49)	Mean if RPO active in collective commercialization (N=20)	Difference
<i>trust outcomes</i>					
efficiency	0.69	0.22	0.66	0.77	-0.11*
farming	0.64	0.21	0.62	0.69	-0.08
general	0.62	0.23	0.59	0.70	-0.11*
information	0.70	0.21	0.67	0.78	-0.11**
integrity	0.70	0.22	0.68	0.77	-0.09
negotiation	0.64	0.21	0.61	0.71	-0.10*
trust_factor	-0.04	0.50	-0.11	-0.15	-0.26*
trust_sum	3.99	1.18	3.82	4.42	-0.61*
<i>additional covariates</i>					
age ^a	55.50	10.44	57.33	51.16	6.18**
bicycles	0.17	0.25	0.14	0.22	-0.07
distance to market	10.38	29.71	11.49	7.68	3.81
distance to storage ^b	11.94	18.56	12.08	11.58	0.51
groundnut	0.41	0.49	0.31	0.65	-0.34***
hectares ^b	3.56	2.88	2.91	5.30	-2.40***
heterogeneity	0.36	0.48	0.31	0.50	-0.19
household size ^b	12.71	4.09	12.58	13.05	-0.46
literate ^b	0.42	0.31	0.40	0.47	-0.08
male	0.42	0.29	0.41	0.46	-0.06
member	0.79	0.15	0.81	0.75	0.06
members' initiative	0.30	0.25	0.30	0.33	-0.03
motorcycles	0.16	0.21	0.13	0.23	-0.10*
onion	0.46	0.50	0.61	0.10	0.51***
phone	0.95	0.07	0.94	0.96	-0.02
related to leader ^b	0.63	0.35	0.64	0.60	0.04
rice	0.13	0.34	0.08	0.25	-0.17*
savings in bank	0.13	0.15	0.10	0.21	-0.11***
savings in MFI	0.32	0.18	0.28	0.40	-0.12***
size ^a	31.83	8.28	31.73	32.05	-0.32
vehicles	0.09	0.19	0.09	0.12	-0.03
year of creation ^a	1996.70	10.30	1996.40	1997.42	-1.02
year joined ^b	1999.53	7.54	1999.54	1999.49	0.05

Notes: Information on collective commercialization in the 2010/2011 season is only available for 69 out of 73 RPOs. We report averages and standard deviations in the sample of 69 RPOs, averages in the subsamples of RPOs that did and did not engage in collective commercialization in the 2010/2011 season and results of two-sample t tests with the null of equal means in RPOs not having engaged and having engaged in collective commercialization.

^a Information on these variables is only available for 64 out of 69 RPOs of which 45 did not engage in collective commercialization in the 2010/2011 season and 19 did.

^b Information on these variables is only available for 62 out of 69 RPOs of which 45 did not engage in collective commercialization in the 2010/2011 season and 17 did.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Descriptive statistics by collective commercialization activity on individual level

Variable	Mean (N=538)	Standard deviation	Mean if farmer not active in collective commercialization (N=467)	Mean if farmer active in collective commercialization (N=71)	Difference
<i>trust outcomes</i>					
efficiency	0.69	0.46	0.67	0.85	-0.18***
farming	0.64	0.48	0.63	0.70	-0.07
general	0.62	0.49	0.61	0.70	-0.10
information	0.70	0.46	0.67	0.86	-0.18***
integrity	0.71	0.46	0.69	0.83	-0.14**
negotiation	0.63	0.48	0.60	0.83	-0.23***
trust_factor	-0.04	1.03	-0.09	0.30	-0.39***
trust_sum	3.99	2.41	3.87	4.77	-0.91***
<i>additional covariates</i>					
age ^a	55.46	9.85	56.16	51.03	5.13***
bicycles	0.15	0.49	0.14	0.21	-0.07
distance to market	12.90	52.43	13.55	8.64	4.91
distance to storage ^b	14.32	24.24	14.22	15.00	-0.78
groundnut	0.47	0.50	0.42	0.82	-0.40***
hectares ^b	4.27	5.27	3.85	6.88	-3.02***
heterogeneity	0.35	0.48	0.31	0.58	-0.27***
household size ^b	12.97	7.02	12.96	13.06	-0.10
literate ^b	0.44	0.50	0.40	0.69	-0.28***
male	0.49	0.50	0.50	0.46	0.03
member	0.75	0.43	0.77	0.62	0.15***
members' initiative	0.31	0.24	0.31	0.32	-0.02
motorcycles	0.16	0.45	0.14	0.28	-0.14**
onion	0.42	0.49	0.47	0.07	0.40***
phone	0.95	0.21	0.95	0.99	-0.04
related to leader ^b	0.65	0.48	0.67	0.53	0.13**
rice	0.12	0.32	0.12	0.11	0.00
savings in bank	0.12	0.32	0.10	0.24	-0.14***
savings in MFI	0.36	0.48	0.32	0.56	-0.24***
size ^a	31.49	6.54	31.56	31.09	0.47
vehicles	0.06	0.33	0.05	0.11	-0.06
year of creation ^a	1997.14	10.49	1997.32	1995.99	1.34
year joined ^b	1999.79	8.90	2000.23	1997.06	3.16***

Notes: Information on collective commercialization in the 2010/2011 season is only available for 538 of 798 farmers belonging to 69 out of 73 RPOs. We report averages and standard deviations for a sample of those 538 farmers, averages in the subsamples of farmers that did and did not engage in collective commercialization in the 2010/2011 season and results of two-sample t tests with the null of equal means among farmers not having engaged and having engaged in collective commercialization.

^a Information on these variables is only available for 493 out of 538 farmers of which 426 did not engage in collective commercialization in the 2010/2011 season and 67 did.

^b Information on these variables is only available for 468 out of 538 farmers of which 404 did not engage in collective commercialization in the 2010/2011 season and 64 did.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Correlation between trust and collective commercialization

Panel A (N=538): Pairwise correlation between different measures of trust and individual participation in collective commercialization									
	negotiation	integrity	information	efficiency	general	farming	trust_sum	trust_factor	
contribution	0.1609***	0.1067**	0.1362***	0.1297***	0.0685	0.0512	0.1271***	0.1271***	
Panel B (N=69): Pairwise correlation between averages of different measures of trust and collective commercialization activity on RPO level									
	negotiation	integrity	information	efficiency	general	farming	trust_sum	trust_factor	
sales	0.2202*	0.1820	0.2452**	0.2327*	0.2297*	0.1709	0.2344*	0.2343*	

Notes: In Panel A we report pairwise correlation coefficients between a binary indicator for individual contribution to collective commercialization in the 2010/2011 season and individual trust measures and, in Panel B, between a binary indicator for having engaged in collective commercialization at RPO level in the 2010/2011 season and averages of trust measures at baseline.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Overall, results from this section support the idea that trust in leaders and in members may play a significant role in fostering collective commercialization. Aiming at enhancing trust within RPOs to encourage collective commercialization, we organized a three-day training and discussion around the potential benefits and difficulties of coordinating towards collective commercialization in partnership with GRET and PINORD.

3 Intervention

We investigate issues of intra-group coordination and trust using a sample of Senegalese village-level RPOs for which we conducted a baseline survey in November and December 2011. At that time people may have assumed that there would be subsequent activities following the survey, but no details on the intervention were given. On the contrary, it was intentionally left unclear that there would be an intervention for some interviewees in the future. The sample for which the baseline survey was conducted was drawn from a dataset of 204 RPOs from 9 federations belonging to FONGS collected in 2009 from which we selected all organizations that stated collective commercialization as one of their main purposes. 73 RPOs belonging to 7 federations satisfied this criterion. Figure 1 shows the location of the sampled RPOs.

We randomly assigned an invitation to the training intervention to those 73 RPOs as follows:

- 23 RPOs served as pure control group, they were not invited to participate in the training intervention
- 50 RPOs received an invitation to participate in the training intervention
 - in 12 RPOs, between one and four leaders but no members were invited to the training
 - in 15 RPOs, between one and four members but no leaders were invited to the training
 - in 23 RPOs, between one and four members and between one and four leaders were invited to the training

In each of the treated RPOs, at least two persons received an invitation. Invitees were randomly selected from lists of members and leaders obtained from the

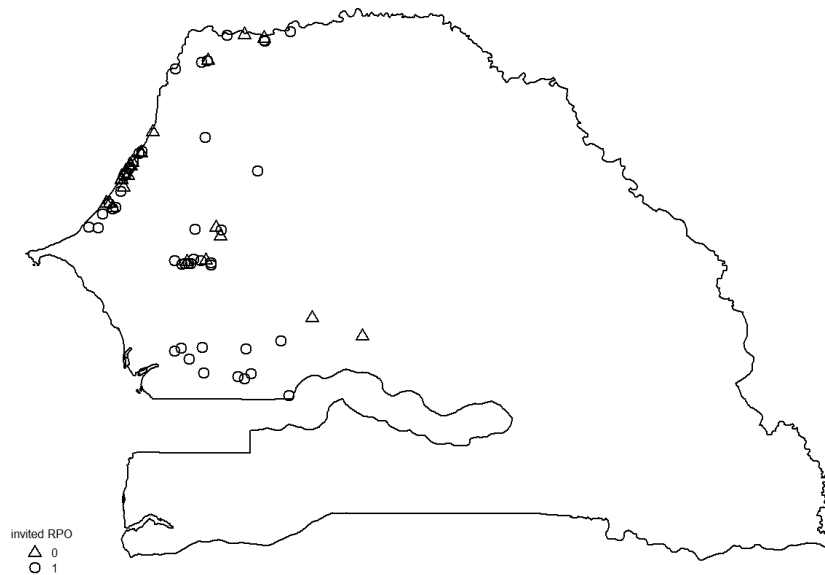


Figure 1: Location of sample RPOs in Senegal.

Map file downloaded from <http://www.mapmakerdata.co.uk.s3-website-eu-west-1.amazonaws.com/library/stacks/Africa/Senegal/index.htm>, retrieved on 6 March 2015.

2009 survey. In all treated RPOs, a general assembly was convened shortly after the training to discuss its content with non-participants. To be able to consider both direct as well as spillover effects, we interviewed both invitees as well as non-invited individuals. In each RPO, 10 non-invited individuals were randomly selected for interviewing purposes.

The treatment and interview assignments were not fully respected in all RPOs. In some RPOs, more than four members (leaders) were invited. There were also instances where some invitees did not participate in the training, where more than the intended number of individuals participated in the training, or where the distribution of members and leaders was not fully respected. Moreover, the number of actually interviewed individuals (including invitees) ranges from 4 to 16 per RPO instead of the targeted 10 to 18. Furthermore, due to missing information on outcome variables, we have to drop 8 individuals belonging to 7 RPOs from our sample. All in all, we work with a sample of 798 interviewed individuals consisting of 150 invitees, 447 non-invited individuals from treated

RPOs, and 201 individuals from control RPOs.¹⁵

The intervention consisted of a three-day training conducted in January 2012. During the training, all participants were treated the same, i.e., members were not treated differently from leaders. The purpose of the training was to create awareness of the (potential) advantage of working together towards commercialization of agricultural produce and highlight particular group dynamics that are conducive to collective action. In terms of content, role plays, simple games and group exercises were to demonstrate the advantages of working together in terms of the benefits that can be generated and the costs that can be saved. Particular focus was put on sharing information and increasing transparency. Participants were encouraged to communicate the message they took away from the training to non-trained fellow farmers from their RPO upon return. One part of the training was to put together a plan for how these messages can be communicated and discussed with other group members.

The training sessions were organized in three modules, one for each day: (1) the potential and pitfalls of collective commercialization, (2) coordination within organizations—the role of communication in collective commercialization, and (3) motivation for coordination—members and leaders.¹⁶ An endline survey was conducted 2 to 4 weeks after the training.

In this paper we focus on estimating immediate effects of the intervention on intra-group relations. We do not capture long-term effects which would also have been interesting considering that trust may need time to grow. We cannot fully preclude a courtesy bias, i.e., that the respondents give the socially desirable answer when reporting higher trust levels after the intervention. However, baseline and endline data were collected via independent interviewers. They were not perceived as being related to GRET and PINORD, the organizations implementing the training, and were ignorant of the treatment status of RPOs as well as individuals. This separation of implementation and data collection should minimize the risk of a courtesy bias. If we found a spillover effect on trust of non-invited individuals in treated RPOs, we would be even less concerned about a courtesy bias. Indeed, our findings suggest that the intervention had an impact on trust of non-invited individuals (see Section 6), thus mitigating concerns related to such

¹⁵See Appendix B.1 for more details on the one-sided noncompliance. The distribution of RPOs and farmers by intended and actual treatment status as shown in Tables B.1 and B.2 in Appendix B.1 underline that treatment assignment is balanced which we further assess in Table 4.

¹⁶The complete training manual is available from the authors upon request.

bias.

Table 4: **Balancing tests at RPO level**

Variable	Mean (N=73)	Standard deviation	Mean in control group (N=23)	Mean in treatment group (N=50)	Difference
<i>trust outcomes</i>					
efficiency	0.70	0.23	0.69	0.70	-0.02
farming	0.64	0.21	0.62	0.65	-0.03
general	0.63	0.23	0.62	0.64	-0.02
information	0.70	0.20	0.73	0.69	0.04
integrity	0.71	0.22	0.72	0.71	0.01
negotiation	0.63	0.20	0.61	0.64	-0.03
trust_factor	-0.03	0.50	-0.04	-0.02	-0.02
trust_sum	4.01	1.17	3.98	4.03	-0.04
<i>additional covariates</i>					
age ^a	55.65	10.18	53.78	56.60	-2.82
bicycles	0.21	0.34	0.15	0.24	-0.09
collective_sales ^b	0.29	0.46	0.18	0.34	-0.16
distance to market	10.16	28.91	5.59	12.26	-6.67
distance to storage ^c	12.88	20.45	5.62	16.51	-10.89**
groundnut	0.38	0.49	0.26	0.44	-0.18
hectares ^c	3.84	4.34	2.64	4.43	-1.79
heterogeneity	0.36	0.48	0.30	0.38	-0.08
household size ^c	12.70	4.01	11.61	13.25	-1.64
literate ^c	0.43	0.31	0.46	0.41	0.05
male	0.41	0.29	0.41	0.42	-0.01
member	0.79	0.15	0.81	0.78	0.03
members' initiative	0.62	0.49	0.52	0.66	-0.14
motorcycles	0.18	0.30	0.14	0.20	-0.07
onion	0.47	0.50	0.57	0.42	0.15
phone	0.95	0.07	0.94	0.95	-0.01
related to leader ^c	0.63	0.35	0.73	0.58	0.15
rice	0.15	0.36	0.17	0.14	0.03
savings in bank	0.14	0.16	0.13	0.15	-0.02
savings in MFI	0.33	0.19	0.30	0.35	-0.05
size ^a	31.38	8.37	31.48	31.33	0.14
vehicles	0.11	0.21	0.13	0.10	0.04
year of creation ^a	1996.69	10.08	1994.96	1997.58	-2.62
year joined ^c	1999.48	7.40	1998.87	1999.78	-0.90

Notes: We report averages and standard deviations for the full sample of 73 RPOs, averages in the subsamples of RPOs in treatment and control group and results of two-sample t tests with the null of equal means in control and treatment group at baseline.

^a Information on these variables is only available for 68 out of 73 RPOs of which 45 belong to the treatment group and 23 to the control group.

^b Information on this variable is only available for 69 out of 73 RPOs of which 47 belong to the treatment group and 22 to the control group.

^c Information on these variables is only available for 66 out of 73 RPOs of which 44 belong to the treatment group and 22 to the control group.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To ensure that the assignment of the treatment is independent of any baseline characteristics, we run a series of balancing tests relating RPO's invitations to participate in the training to individual and RPO-level characteristics. In Table 4 we report means of individual and RPO-level characteristics in invited and non-invited RPOs at baseline as well as results from two-sample t tests with the null of equal means in invited and non-invited RPOs. Results suggest that the treatment

allocation is fully independent of average baseline trust levels,¹⁷ and a range of further covariates. Treatment and control group only differ significantly with respect to the average walking distance of the farmers to the RPO's storage site.¹⁸ Multivariate tests for equality of means of baseline trust in invited and non-invited groups reveal no significant differences between treatment and control groups.¹⁹ Within invited RPOs, we find no significant differences in average baseline trust when comparing invitees and non-invitees (see Table 5 for details on binary trust measures and Table B.4 in Appendix B.2 for details on ordinal trust measures).

¹⁷See Table B.3 in Appendix B.2 for balancing tests using ordinal trust measures.

¹⁸Information on this variable is only available for 66 out of 73 RPOs.

¹⁹Wilks' lambda statistic 0.8651, Pillai's trace statistic 0.1349, Lawley-Hotelling trace statistic and Roy's largest root statistic 0.1560, p-value 0.1311.

Table 5: Changes in trust by intended treatment status at individual level

Trust outcomes	Sample	N	Baseline (BL)	Endline (EL)	Diff. BL EL P-value
negotiation	A. Invitees	150	0.68	0.77	0.0940
	B. Non-Invitees in T	447	0.63	0.75	0.0001
	C. Non-Invitees in C	201	0.61	0.62	0.8379
	Diff. A B P-value		0.2999	0.7531	
	Diff. B C P-value		0.6070	0.0006	
integrity	A. Invitees	150	0.74	0.79	0.3433
	B. Non-Invitees in T	447	0.69	0.69	0.9423
	C. Non-Invitees in C	201	0.71	0.59	0.0089
	Diff. A B P-value		0.2807	0.0251	
	Diff. B C P-value		0.6459	0.0096	
information	A. Invitees	150	0.71	0.82	0.0209
	B. Non-Invitees in T	447	0.68	0.74	0.0325
	C. Non-Invitees in C	201	0.72	0.68	0.3290
	Diff. A B P-value		0.5117	0.0546	
	Diff. B C P-value		0.2677	0.0824	
efficiency	A. Invitees	150	0.75	0.83	0.0657
	B. Non-Invitees in T	447	0.68	0.70	0.6131
	C. Non-Invitees in C	201	0.68	0.64	0.3448
	Diff. A B P-value		0.1378	0.0012	
	Diff. B C P-value		0.9852	0.1232	
general	A. Invitees	150	0.64	0.78	0.0074
	B. Non-Invitees in T	447	0.62	0.65	0.4043
	C. Non-Invitees in C	201	0.61	0.54	0.1906
	Diff. A B P-value		0.7289	0.0032	
	Diff. B C P-value		0.6773	0.0084	
farming	A. Invitees	150	0.66	0.78	0.0206
	B. Non-Invitees in T	447	0.64	0.67	0.2913
	C. Non-Invitees in C	201	0.61	0.56	0.3123
	Diff. A B P-value		0.6556	0.0136	
	Diff. B C P-value		0.4970	0.0064	
trust_sum	A. Invitees	150	4.17	4.77	0.0162
	B. Non-Invitees in T	447	3.95	4.21	0.1062
	C. Non-Invitees in C	201	3.95	3.63	0.1730
	Diff. A B P-value		0.3311	0.0087	
	Diff. B C P-value		0.9785	0.0036	
trust_factor	A. Invitees	150	0.04	0.29	0.0168
	B. Non-Invitees in T	447	-0.05	0.05	0.1178
	C. Non-Invitees in C	201	-0.05	-0.19	0.1652
	Diff. A B P-value		0.3266	0.0080	
	Diff. B C P-value		0.9994	0.0041	

Notes: We report averages of different trust measures in subsamples of invitees, non-invitees in the treatment group (T—treatment group), non-invitees in the control group (C—control group) at baseline (BL—baseline) and endline (EL—endline) as well as p-values of two-sample t tests with the null of equal means.

4 Experimental design

4.1 Hypotheses

Ostrom and Ahn (2003) provide a theoretical framework for analyzing issues of collective action which can be applied to analyzing collective commercialization as an example of collective action. Success and failure of collective action is determined by a complex configuration of various factors which they categorize as forms of social capital. They argue that the different forms of social capital—trustworthiness, networks and institutions—enhance trust which they see as the core link between social capital and collective action. Figure 2 shows the relationships between the forms of social capital, trust, and collective action.

Forms of Social Capital

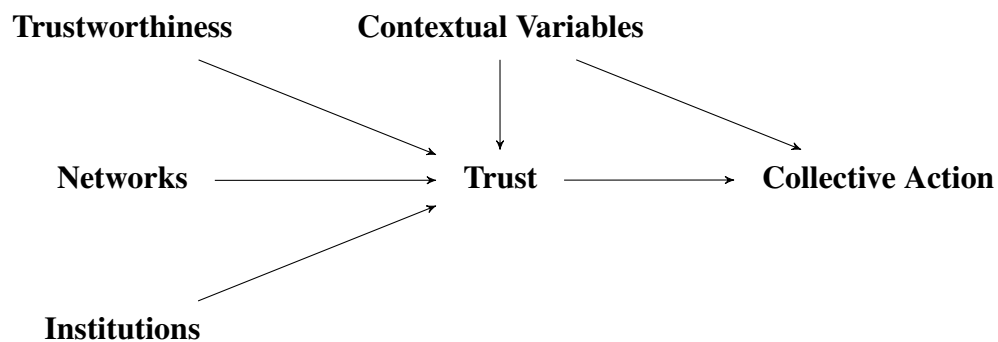


Figure 2: **Social capital, trust, and collective action.**
(Ostrom and Ahn, 2003, p. xvii)

Trust reduces opportunistic behavior and, thus, transaction costs (Collier, 2002; Putnam, 1993). Applied to the context of collective commercialization in RPOs, farmers who are organized in RPOs need to be trustworthy and sufficiently connected with each other, and the RPO needs to provide adequate institutions for trust to be prevalent within the RPO and for collective commercialization to be successful. Trust in leaders allows the trusting farmer to commit to commercialization despite the risk of loss if the leaders do not behave as the trusting farmer expected. Trust in members allows the trusting farmer to commit to commercialization despite the risk of loss if the fellow farmers do not participate in the joint endeavors as expected by the trusting farmer.

If the training intervention increases the different forms of social capital, it

will enhance trust and ultimately lead to more successful collective commercialization and potentially other collective endeavors in treated RPOs. Putnam (1993) distinguishes between horizontal networks bringing together individuals of equivalent status, e.g. fellow members or fellow leaders, and vertical networks bringing together individuals from different hierarchical levels, e.g. members and leaders. Dense horizontal networks with the capability of efficiently transmitting information across individuals create incentives to behave in a trustworthy manner. Repeated interactions among individuals are seen as a sign of robust networks (Ostrom and Ahn, 2003). We expect that the training intervention strengthens existing horizontal and vertical networks and may even create new ones through repeated interactions of farmers. Put differently, we expect the intervention to have a positive effect on participating leaders' trust in other leaders and on participating members' trust in other members through tightening existing bonds and creating new ones during and after the joint field trip to the training. Similarly, in RPOs where both members and leaders participate, the training may increase trust between leaders and members through strengthening and/or creating vertical networks. We hypothesize that such increases in social capital will materialize in a positive treatment effect on trust in members and on trust in leaders.

Both networks and institutions change the incentive structure of individuals with institutions creating incentives for both the trusting and the trusted party to behave in a trustworthy manner (Ostrom and Ahn, 2003). We expect that the intervention clarifies the process of collective commercialization as one form of collective action and the rules involved and, thus, creates a common understanding of the relevant institutions. In other words, trained members (leaders) may obtain a better understanding of leaders' (members') role and position which may increase members' (leaders') trust in leaders (members) even if no leaders (members) are present during the training. Further, the intervention may also create positive spillover effects, i.e., increase non-participants' trust in leaders and members. Participants may convey their newly acquired understanding of leaders' and members' role and position to non-participants during the general assembly held shortly after the training. We hypothesize that such increases in social capital will again materialize in a positive treatment effect on trust in members and on trust in leaders.

4.2 Measures of trust

We consider six survey measures of trust capturing different aspects of trust within a RPO that we consider to be important for successful collective commercialization. We distinguish between trust in leaders and trust in members.

Measuring general trust²⁰ in unknown others based on the standard trust question used in the General Society Survey (GSS)²¹ or the World Values Survey²² has been challenged on the grounds that it is unclear what this question actually measures (Beugelsdijk, 2006; Durlauf, 2002; Sobel, 2002; Yamagishi et al., 1999). Results from relating general trust in strangers as stated in the survey question to behavior in experimental trust games are mixed.²³

However, survey measures are commonly used for studying trust as a collective property of organizations, i.e., for studying trust in known others. In the management literature, the perception of the characteristics of significant others within the working context is looked at for studying trust between and within organizational levels (Tschannen-Moran and Hoy, 2000). We borrow from this literature to assess how characteristics of members and leaders are perceived within the RPO context.²⁴ For trust in leaders, we distinguish between trust in their motives and trust in their competence with regard to collective commercialization. Trust in leaders' motives refers to a farmer's beliefs that the leaders are other-regarding or not, i.e., that the leaders have motives to perform an action beneficial to others or not. Trust in leaders' competence refers to a farmer's beliefs that the leaders have credible expertise (Buck and Alwang, 2011; Twyman et al., 2008).

²⁰General trust is a baseline expectation of others' trustworthiness (Yamagishi, 2001).

²¹"Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?"—"Most people can be trusted." or "Can't be too careful."

²²"Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?"—"Most people can be trusted." or "Need to be very careful."

²³Glaeser et al. (2000) find in a sample of students at Harvard University that trust as stated in the survey has no predictive power for trust elicited from the trust game, but for trustworthiness. For a representative sample of the German population, Fehr et al. (2002) find a relation of trust as measured in the survey and experimental trust, but not for trustworthiness. Sapienza et al. (2007) find that trust as stated in the survey predicts experimental trust for a sample of MBA students at the University of Chicago. For a heterogeneous Dutch sample, Bellemare and Kröger (2007) find no significant link between trust as measured in the survey to neither experimental trust nor trustworthiness. Naef and Schupp (2009) have developed a new survey measure for trust in strangers that is more precise than the GSS question and have used it in the German Socio-Economic Panel showing that survey and experimental measures of trust are connected in the sense that the trust game measures trust in strangers.

²⁴See Kramer (1999) and Möllering et al. (2004) for reviews of the conceptualization of trust in organizational theory and the benefits of and conditions for trust in organizational systems. See Seppänen et al. (2007) for a review of the empirical research of inter-organizational trust.

We consider four aspects of trust in leaders asking respondents to which extent they agree with the following statements:

- (1) “The RPO’s board is capable of negotiating better prices for our produce than I am capable of myself.”
- (2) “Board members defend the RPO’s interests and their own interests in equal measure.”
- (3) “If the RPO’s board says that I can achieve better prices if I wait a little, they do so because they have adequate information.”
- (4) “If I invest my money or my produce in the RPO, it will be used efficiently.”

For trust in members, we consider both general trust and trust specific to the farming context asking respondents to which extent they agree with the following statements:

- (5) “The majority of people in my RPO are trustworthy.”
- (6) “I can trust the people from the RPO to look after my field during an absence of two months.”

Originally, all trust measures are recorded on a four-point Likert-type scale. The items to choose from are (1) “Do not agree at all”, (2) “Do not really agree”, (3) “Agree”, and (4) “Fully agree”. However, given that the average trust levels at baseline are well above 3 for all measures of trust (see Table B.4 in Appendix B.2), we use binary variables as regressands in the empirical analysis which are equal to one if respondents fully agree with one of the above statements.

Hence, for trust in leaders, we create a binary variable *negotiation* equal to one if the respondent fully agrees with the first statement, a binary variable *integrity* equal to one if the respondent fully agrees with the second statement, a binary variable *information* equal to one if the respondent fully agrees with the third statement, and a binary variable *efficiency* equal to one if the respondent fully agrees with the fourth statement. For trust in members, we create a binary variable *general* equal to one if the respondent fully agrees with the fifth statement, and a binary variable *farming* equal to one if the respondent fully agrees with the sixth statement.

Moreover, we create two comprehensive indices of trust. For *trust_sum* we take the sum over the six binary trust measures such that values of *trust_sum*

range from 0 to 6.²⁵ For *trust_factor* we run a factor analysis with the six binary trust measures. Values of the resulting index of trust range from -1.74 to 0.81.²⁶

5 Treatment effects at RPO level

In this section we present our empirical strategy and regression results for the causal treatment effects of the (invitation to the) training on average trust.

Table 6: **Changes in trust by intended treatment status at RPO level**

Trust outcomes	Sample	N	Baseline (BL)	Endline (EL)	Diff. BL EL P-value
negotiation	Control (C)	23	0.61	0.63	0.8413
	Treatment (T)	50	0.64	0.76	0.0029
	Diff. C T P-value		0.5877	0.0113	
integrity	Control (C)	23	0.72	0.60	0.1156
	Treatment (T)	50	0.71	0.72	0.7788
	Diff. C T P-value		0.8566	0.0783	
information	Control (C)	23	0.73	0.69	0.5575
	Treatment (T)	50	0.69	0.77	0.0670
	Diff. C T P-value		0.3965	0.2044	
efficiency	Control (C)	23	0.69	0.66	0.6719
	Treatment (T)	50	0.70	0.73	0.5387
	Diff. C T P-value		0.7572	0.2345	
general	Control (C)	23	0.62	0.56	0.4360
	Treatment (T)	50	0.64	0.69	0.3513
	Diff. C T P-value		0.7215	0.0708	
farming	Control (C)	23	0.62	0.57	0.5411
	Treatment (T)	50	0.65	0.70	0.2368
	Diff. C T P-value		0.5602	0.0558	
trust_sum	Control (C)	23	3.98	3.71	0.4762
	Treatment (T)	50	4.03	4.38	0.1843
	Diff. C T P-value		0.8838	0.0657	
trust_factor	Control (C)	23	-0.04	-0.15	0.4682
	Treatment (T)	50	-0.02	0.12	0.1944
	Diff. C T P-value		0.8976	0.0691	

Notes: We report averages of different trust measures in treatment group (T—treatment group) and control group (C—control group) at baseline (BL—baseline) and endline (EL—endline) as well as p-values of two-sample t tests with the null of equal means.

Descriptive evidence suggests a positive impact of the training intervention on intra-RPO trust. Table 6 reports two-sample t tests with the null of equal means in baseline and endline for treatment and control group. Moreover, we report two-sample t tests with the null of equal means for treatment and control group in both baseline and endline. Treatment RPOs report higher levels of average trust

²⁵*trust_sum* has a mean of 4.08 and a standard deviation of 2.35.

²⁶*trust_factor* has a mean of 0 and a standard deviation of 1.

at endline compared to baseline for all measures of trust. We observe statistically significant differences between average trust in leaders’ negotiation skills and their informational advantages. While we find no significant differences between average trust in treatment and control group at baseline, treatment RPOs report significantly higher levels of trust at endline compared to control RPOs for trust in leaders’ negotiation skills and their integrity, for general and farming-specific trust in members, and for both comprehensive trust indices.²⁷

5.1 Empirical strategy

To further study the intervention’s impact on trust, we estimate treatment effects on RPO level. To account for the one-sided noncompliance²⁸ that may be related to individual and RPO-level characteristics, we first present estimations of the intention-to-treat effect, i.e., the treatment effect of the invitation to participate in the training intervention. We draw on the random assignment of the invitations (and the time dimension of our data) to identify the causal effect of such an invitation at the RPO level on our different measures of trust.

In our main regressions we will allow for fixed effects to pick up differences in unobservable characteristics between individuals and/or RPOs. Although treatment was randomly assigned, the relatively small number of RPOs may entail some small sample imbalances in observed and/or unobserved characteristics. To be on the safe side, we include fixed effects. Since our analysis is based on two time periods, fixed effects regressions are identical to taking first differences in a regression with no further covariates.²⁹ Thus, we estimate:

$$\Delta trust_g = \gamma_0 + \gamma_1 RPO_invitation_g + \nu_g \quad (1)$$

where $\Delta trust_g = trust_{g,t=1} - trust_{g,t=0}$ is the difference between average trust

²⁷For descriptive evidence on the intervention’s impact on ordinal trust measures see Table B.4 in Appendix B.2.

²⁸Participation in the training intervention was voluntary among those who were invited, but no one from the control RPOs had access to the training intervention. For details on the one-sided noncompliance see Appendix B.1.

²⁹Double differencing not only removes biases in an endline comparison between the treatment and control group resulting from permanent differences, but also biases from a comparison over time in the treatment group resulting from time trends that are unrelated to the intervention. The key identifying assumption (common trend assumption) for the estimation of a causal treatment effect is that trends in development of trust would on average be the same in treatment and control groups in the absence of the training intervention (Imbens and Wooldridge, 2009).

in RPO g at endline $t = 1$ and baseline $t = 0$ and $RPO_invitation_g$ is a binary variable equal to one if at least one member or leader of RPO g has received an invitation to participate in the training intervention. ν_g is a random disturbance term for RPO g . Given the random assignment of the invitations, the estimated coefficient $\widehat{\gamma}_1$ is our main coefficient of interest, the causal effect of the invitation to the training on average trust. It captures both the treatment effect for those individuals who have personally been invited (direct treatment effect) and those who have only been interviewed (spillover effect) which we will disentangle in Section 6.

For illustrative purposes, we also report results of level regressions, i.e., of regressions without fixed effects. Using endline data, we estimate the following linear model:

$$trust_{g,t=1} = \beta_0 + \beta_1 RPO_invitation_g + \varepsilon_g \quad (2)$$

where $trust_{g,t=1}$ is a measure of average trust in RPO g at endline $t = 1$ and $RPO_invitation_g$ is defined as above. Hence, $\widehat{\beta}_1$ is our main coefficient of interest in this model, the average causal effect of the invitation to the training on average trust. ε_g is a random disturbance term for RPO g .

To address concerns with regard to statistical inference in the small cross-section sample of 73 RPOs, we provide both OLS standard errors and classical inference as well as randomization inference. For the intention-to-treat estimations based on equations (1) and (2), we implement permutation tests which do not rely on asymptotic approximation and whose properties are hence independent of sample size.³⁰ Randomization inference is based on the sharp null hypothesis of no treatment effect for anybody and provides exact finite sample inference. We conduct randomization inference by forming 9000 randomly drawn permutations of the randomization vector for the 73 RPOs and calculating the regression estimates using regressions as presented in equations (1) and (2). For each outcome of interest, this provides the finite sample distribution of the estimated treatment effect under the null of no effect, upon which we base our two-sided test of zero effect. For recent applications of this approach see for example Bloom et al.

³⁰Randomization inference is typically only done for intention-to-treat estimations. Randomization inference in the instrumental variable regression context would require modeling assumptions since the compliance type is not known for all observations, i.e. the counterfactual value of $RPO_training$ if $RPO_invitation$ was changed.

(2013) and Landmann and Frölich (2015).

In next step, we estimate the effect of treatment on the treated, i.e., the causal effect of the training intervention on those who participated, using the randomly assigned invitations as an instrumental variable for training received. We estimate:

$$trust_{g,t=1} = \delta_0 + \delta_1 RPO_training_g + v_g \quad (3)$$

$$\Delta trust_g = \eta_0 + \eta_1 RPO_invitation_g + \omega_g \quad (4)$$

where $RPO_training_g$ is a binary variable equal to one if at least one member or leader of RPO g participated in the training which we instrument for by $RPO_invitation_g$. The first stage regression coefficient is 0.88. Since both the treatment variable and the instrument are binary, the instrumental variable regression is identical to the Wald (or LATE) estimator. The estimated effects are identical to those of equations (1) and (2) divided by 0.88. We therefore report randomization inference only for the reduced form regressions, estimates only differ by this scaling factor. The F statistic from the first-stage regressions is 351.60, the adjusted R^2 is 0.6937. $trust_{g,t=1}$ and $\Delta trust_g$ are defined as above. v_g and ω_g are random disturbance terms for RPO g . Thus, the estimated coefficients $\hat{\delta}_1$ and $\hat{\eta}_1$ are our main coefficients of interest, the causal effects of the training on average trust in those RPOs who participated.

5.2 Regression results

In Table 7 we report estimated treatment effects on average trust. In panel A we report results from regressions using endline data while panel B shows results from first differences estimations. We present intention-to-treat effects based on reduced form regressions as specified in equations (1) and (2), and we present treatment on the treated effects based on instrumental variable regressions as specified in equations (3) and (4).

The regression results confirm the descriptive evidence presented above. An invitation to the training significantly increases both comprehensive trust measures. The estimated treatment effect becomes even larger when looking at the effect of the training itself on average trust. In detail, we find a significantly positive intention-to-treat effect of between 11 and 13 percentage points for average trust in leaders' negotiation skills, their integrity and their informational advan-

tage.³¹ We further find a significantly positive intention-to-treat effect of between 10 and 13 percentage points for both measures of trust in members. These positive treatment effects increase to 12 to 15 percentage points when considering the effect of the training itself.

We run several robustness checks to confirm these results. First, we enlarge the model in first differences by allowing for onion-specific and rice-specific time trends and by controlling for the number of invited/trained persons per RPO.³² Qualitatively, results remain unchanged compared to the model in first differences without covariates. Second, we follow McKenzie (2012) who argues that large improvements in power can arise when estimating treatment effects via an Analysis of Covariance (ANCOVA) estimation compared to using the difference-in-differences specification. We estimate the ANCOVA specifications without covariates, with controls for onion and rice cultivation and with a control for the number of invited/trained persons per RPO. Results are similar to those from the difference-in-differences estimations and robust to controlling for onion and rice cultivation and to controlling for the number of invitees/participants. For details on these robustness checks see Appendix B.3.

³¹Recall that baseline trust averages range between 0.63 for trust in leaders' negotiation skills and general trust in members and 0.71 for trust in leaders' integrity (see Table 4.)

³²Note that when including covariates to the model in first differences, it is no longer equivalent to a fixed effects model.

Table 7: Treatment effects at RPO level

	(1) negotiation	(2) integrity	(3) information	(4) efficiency	(5) general	(6) farming	(7) trust_sum	(8) trust_factor
Panel A: outcome variable <i>trust</i>								
<i>Reduced form regressions (conventional and randomized inference)</i>								
RPO_invitation	0.1333** (0.0546)	0.1154** (0.0640)	0.0770 (0.0585)	0.0779 (0.0643)	0.1258** (0.0682)	0.1323** (0.0695)	0.6617** (0.3514)	0.2786** (0.1497)
[p-value conventional inference]	{0.0171}	{0.0755}	{0.1924}	{0.2298}	{0.0693}	{0.0608}	{0.0637}	{0.0669}
[p-value randomized inference]	{0.0138}	{0.0809}	{0.2073}	{0.2413}	{0.0802}	{0.0609}	{0.0733}	{0.0789}
<i>Instrumental variable regressions (conventional inference)</i>								
RPO_training	0.1515** (0.0608)	0.1311* (0.0709)	0.0875 (0.0650)	0.0885 (0.0713)	0.1429* (0.0749)	0.1504** (0.0766)	0.7520* (0.3878)	0.3166* (0.1653)
[p-value conventional inference]	{0.0126}	{0.0644}	{0.1783}	{0.2144}	{0.0564}	{0.0496}	{0.0525}	{0.0554}
Panel B: outcome variable $\Delta trust$								
<i>Reduced form regressions (conventional and randomized inference)</i>								
RPO_invitation	0.1051 \times (0.0645)	0.1257** \times (0.0649)	0.1208** \times (0.0648)	0.0602 (0.0733)	0.1048** \times (0.0614)	0.1017 \times (0.0676)	0.6182** \times (0.3488)	0.2623** \times (0.1489)
[p-value conventional inference]	{0.1075}	{0.0566}	{0.0662}	{0.4143}	{0.0922}	{0.1368}	{0.0805}	{0.0823}
[p-value randomized inference]	{0.0575}	{0.0202}	{0.0367}	{0.2902}	{0.0555}	{0.0707}	{0.0331}	{0.0340}
<i>Instrumental variable regressions (conventional inference)</i>								
RPO_training	0.1194 (0.0729)	0.1428* (0.0732)	0.1373* (0.0730)	0.0684 (0.0821)	0.1191* (0.0686)	0.1156 (0.0757)	0.7025* (0.3923)	0.2981* (0.1674)
[p-value conventional inference]	{0.1014}	{0.0510}	{0.0602}	{0.4051}	{0.0826}	{0.1271}	{0.0733}	{0.0750}
N	73	73	73	73	73	73	73	73

Notes: In panel A we report coefficients from regressions using endline data regressing trust on a constant and a binary treatment indicator. In panel B we report coefficients from regressions using both baseline and endline data regressing changes in trust on a constant and a binary treatment indicator. For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation* as instrument for *RPO_training*. Standard errors are reported in parentheses (clustered at RPO level). p-values for conventional inference are reported in brackets, p-values for randomized inference with 9000 random permutations are reported in curly brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, for conventional inference

\times $p < 0.10$, \times $p < 0.05$, \times $p < 0.01$, for randomized inference

6 Treatment effects at individual level

In this section we present treatment effects of the (invitation to the) training on trust based on individual-level regressions permitting us to disentangle the effect of having personally been invited from a potential spillover effect on non-invited farmers within treated RPOs. We start by assessing descriptively whether trust has evolved between the baseline and endline survey and whether it has evolved differently in treatment and control groups. Table 5 reports two-sample t tests with the null of equal means in baseline and endline for invitees, non-invitees in invited RPOs and for non-invitees in non-invited RPOs. Moreover, we report two-sample t tests with the null of equal means for invitees and non-invitees in invited RPOs in both baseline and endline and for non-invitees in invited and non-invited RPOs in both baseline and endline. Invitees report higher levels of trust at endline compared to baseline for all measures for trust. This difference is statistically significant for all measures except for trust in leaders' integrity. Further indication of a positive direct treatment effect comes from the comparison of invitees with non-invitees from invited RPOs. While we observe no significant difference at baseline, we observe significant differences for all measures of trust in leaders but *negotiation* at endline. We also find descriptive evidence for a positive spillover effect within invited RPOs. Comparing non-invitees from invited and non-invited RPOs, we find no significant differences at baseline. At endline, however, non-invitees in invited RPOs report significantly higher trust levels for all trust measures but *efficiency* compared to non-invitees from non-invited RPOs.

6.1 Empirical strategy

To further study spillover effects and direct treatment effects, we estimate:

$$\begin{aligned} \Delta trust_{ig} = & \alpha_0 + \alpha_1 spillover_invitation_{ig} + \alpha_2 personal_invitation_i \\ & + \tau_{ig} \end{aligned} \tag{5}$$

where $\Delta trust_{ig} = trust_{ig,t=1} - trust_{ig,t=0}$ is the difference between trust of person i in RPO g at endline $t = 1$ and baseline $t = 0$, $spillover_invitation_{ig}$ is a binary variable equal to one if some member or leader from RPO g other than person i has received an invitation, $personal_invitation_i$ is a binary variable equal to one if person i has received an invitation to participate in the training

intervention, and τ_{ig} is a random disturbance term for individual i in RPO g . Hence, the estimated coefficient $\widehat{\alpha}_1$ captures any potential spillover effects while $\widehat{\alpha}_2$ captures the effect of having personally been invited.

Again, we also estimate the causal effect of the training using an instrumental variable strategy. We estimate:

$$\Delta trust_{ig} = \theta_0 + \theta_1 spillover_training_{ig} + \theta_2 personal_training_i + \kappa_{ig} \quad (6)$$

where $spillover_training_{ig}$ is a binary variable equal to one if some member or leader from RPO g other than person i has participated in the training and $personal_training_i$ is a binary variable equal to one if person i has participated in the training intervention. We instrument for $spillover_training_{ig}$ and $personal_training_i$ using $RPO_invitation_g$ and $personal_invitation_i$.³³ The F statistic from the first-stage regression of $spillover_training_{ig}$ amounts to 174.18, the adjusted R^2 to 0.6062. For $personal_training_i$ the F statistic is 240.95 and the adjusted R^2 is 0.6403. κ_{ig} is a random disturbance term for individual i in RPO g , and $\Delta trust_{ig}$ is defined as above. Hence, the estimated coefficient $\widehat{\theta}_1$ captures any potential spillover effects while $\widehat{\theta}_2$ captures the effect of having personally been trained.

To gain further insights into spillover effects, we also estimate regressions as specified in equation (6) separately for subsamples containing only members or only leaders. For the subsample of members, we further split the spillover effect by treatment type, i.e., by whether only members were treated, only leaders, or both. In these regressions, we instrument for $spillover_training_{ig}$ and $personal_training_i$ by $RPO_invitation_g$ and $personal_invitation_i$.³⁴ We instrument for $spillover_training_OL_{ig}$, $spillover_training_OM_{ig}$, $spillover_training_LM_{ig}$ and $personal_training_i$ using $RPO_invitation_OL_g$, $RPO_invitation_OM_g$, $RPO_invitation_LM_g$ and $personal_invitation_i$. The highly significant correlation coefficients range between -0.2863 and 0.9021.

³³The highly significant correlation coefficients are 0.5702 for $spillover_training$ and $RPO_invitation$, 0.2700 for $personal_training$ and $RPO_invitation$, -0.3509 for $personal_invitation$ and $spillover_training$ and 0.7993 for $personal_invitation$ and $personal_training$.

³⁴The highly significant correlation coefficients are 0.6328 for $spillover_training$ and $RPO_invitation$, 0.2190 for $personal_training$ and $RPO_invitation$, -0.2940 for $personal_invitation$ and $spillover_training$ and 0.7813 for $personal_invitation$ and $personal_training$.

6.2 Regression results

In Table 8 we report estimated treatment effects on individual trust. We present intention-to-treat effects based on reduced form regressions as specified in equation (5), and results from instrumental variable estimations as specified in equation (6). Results hint at a significantly positive treatment effect of having personally received an invitation. In detail, we find a significant personal intention-to-treat effect of 13 to 20 percentage points for all individual trust measures but trust in leaders' negotiation skills.³⁵ These effects increase to 15 to 24 percentage points when considering the treatment effect of the training.

To gain further insights into spillover effects and direct treatment effects, we rerun estimations as specified in equations (5) and (6) on subsamples containing only members or only leaders. Results are presented in Tables 9 (members) and 10 (leaders). In Table 9 we present intention-to-treat effects based on reduced form regressions on a subsample of members as specified in equation (5) and results from instrumental variable estimations on a subsample of members as specified in equation (6). For members we find both a significantly positive personal treatment effect as well as a significantly positive spillover effect. In particular, we find significantly positive direct treatment effects of 19 to 20 percentage points for trust in leaders' integrity and their informational advantage as well as a significant personal intention-to-treat effect of 18 percentage points on general trust and farming specific trust in members. For trust in leaders' negotiation skills, their integrity, and their informational advantages, results appear to be indicating a significantly positive spillover effect of 15 to 16 percentage points. This may be attributed to untrained members believing that trained leaders have gained relevant skills (trust in competence) and that trained leaders are less likely to privately capture rents (trust in motives). We further split these spillover effects into spillovers in RPOs where only leaders were invited (participated), RPOs where only members were invited (participated) and RPOs where both leaders and members were invited (participated). Results presented in Table 9 hint at the spillover effects in trust in leaders being most pronounced in RPOs where either only leaders or only members were invited (participated). Given the small sample size in the subsample containing only leaders (N=168), results for this subsample present inconclusive (see Table 10).

³⁵The average baseline trust levels range from 0.62 for general trust in members to 0.71 for trust in leaders' integrity.

Table 8: Treatment effects at individual level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ negotiation	Δ integrity	Δ information	Δ efficiency	Δ general	Δ farming	Δ trust_sum	Δ trust_factor
<i>Reduced form regressions</i>								
spillover_invitation	0.1109 (0.0686)	0.1221* (0.0692)	0.1097 (0.0702)	0.0604 (0.0778)	0.0915 (0.0649)	0.0833 (0.0721)	0.5779 (0.3792)	0.2451 (0.1618)
personal_invitation	0.0767 (0.0765)	0.1710** (0.0723)	0.1581** (0.0753)	0.1314* (0.0786)	0.2047** (0.0786)	0.1698** (0.0811)	0.9117** (0.3946)	0.3892** (0.1682)
<i>Instrumental variable regressions</i>								
spillover_training	0.1289 (0.0792)	0.1362* (0.0795)	0.1220 (0.0808)	0.0643 (0.0887)	0.0970 (0.0735)	0.0894 (0.0820)	0.6378 (0.4332)	0.2703 (0.1849)
personal_training	0.0743 (0.0852)	0.1891** (0.0782)	0.1754** (0.0829)	0.1516* (0.0841)	0.2365*** (0.0873)	0.1948** (0.0890)	1.0217** (0.4248)	0.4364** (0.1808)
N	798	798	798	798	798	798	798	798

Notes: The number of invited/trained persons in treated RPOs ranges from 2 to 16. Hence, it is impossible to be the only invited/treated person in a treated RPO. We report coefficients from regressions using both baseline and endline data regressions changes in trust on a constant, a binary indicator for personal treatment and a binary indicator for treatment of others. For instrumental variable regressions we report coefficients from two-stage estimations using $RPO_invitation$ and $personal_invitation$ as instruments for $spillover_training$ and $personal_training$. Standard errors are reported in parentheses (clustered at RPO level).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Treatment effects at individual level—subsample of members

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ negotiation	Δ integrity	Δ information	Δ efficiency	Δ general	Δ farming	Δ trust_sum	Δ trust_factor
<i>Reduced form regressions</i>								
spillover_invitation	0.1614** (0.0709)	0.1575** (0.0701)	0.1508** (0.0694)	0.0979 (0.0823)	0.0805 (0.0667)	0.0849 (0.0710)	0.7330* (0.3758)	0.3118* (0.1605)
personal_invitation	0.1351 (0.0977)	0.1881** (0.0806)	0.2020** (0.0843)	0.1342 (0.0967)	0.1838** (0.0801)	0.1778** (0.0848)	1.0211** (0.4289)	0.4351** (0.1830)
<i>Instrumental variable regressions</i>								
spillover_training	0.1873** (0.0831)	0.1801** (0.0817)	0.1714** (0.0808)	0.1111 (0.0945)	0.0879 (0.0762)	0.0934 (0.0813)	0.8311* (0.4338)	0.3535* (0.1852)
personal_training	0.1381 (0.1165)	0.2075** (0.0917)	0.2271** (0.0973)	0.1514 (0.1093)	0.2196** (0.0923)	0.2108** (0.0965)	1.1545** (0.4817)	0.4921** (0.2055)
<i>Reduced form regressions</i>								
spillover_invitation_OL	0.1677* (0.0850)	0.1859** (0.0810)	0.1414 (0.0915)	0.0747 (0.0934)	0.0626 (0.0764)	0.1172 (0.0796)	0.7495* (0.4222)	0.3181* (0.1806)
spillover_invitation_OM	0.1829** (0.0832)	0.1844** (0.0880)	0.1771* (0.0920)	0.1193 (0.1018)	0.1072 (0.0830)	0.0839 (0.0830)	0.8549* (0.4717)	0.3639* (0.2017)
spillover_invitation_LM	0.1429* (0.0814)	0.1225* (0.0732)	0.1382* (0.0743)	0.0967 (0.0864)	0.0727 (0.0747)	0.0667 (0.0750)	0.6397 (0.3978)	0.2723 (0.1696)
personal_invitation	0.1351 (0.0979)	0.1881** (0.0807)	0.2020** (0.0844)	0.1342 (0.0968)	0.1838** (0.0803)	0.1778** (0.0849)	1.0211** (0.4296)	0.4351** (0.1833)
<i>Instrumental variable regressions</i>								
spillover_training_OL	0.2000* (0.1054)	0.2217** (0.1013)	0.1687 (0.1086)	0.0892 (0.1080)	0.0747 (0.0878)	0.1398 (0.0903)	0.8940* (0.4939)	0.3795* (0.2112)
spillover_training_OM	0.3245* (0.1690)	0.3279** (0.1515)	0.3307* (0.1809)	0.2105 (0.1800)	0.2197 (0.1466)	0.1780 (0.1567)	1.5914* (0.8499)	0.6769* (0.3630)
spillover_training_LM	0.1282 (0.0876)	0.1030 (0.0737)	0.1113 (0.0780)	0.0834 (0.0859)	0.0434 (0.0775)	0.0382 (0.0764)	0.5075 (0.4039)	0.2162 (0.1720)
personal_training	0.1306 (0.1153)	0.2008** (0.0904)	0.2176** (0.0958)	0.1446 (0.1078)	0.2113* (0.0910)	0.2080** (0.0944)	1.1129** (0.4756)	0.4743** (0.2028)
N	630	630	630	630	630	630	6308	630

Notes: We report coefficients from regressions on the subsample of members using both baseline and endline data regressing changes in trust on a constant, a binary indicator for personal treatment and a binary indicator for treatment of others (binary indicators for treatment of others by group type). For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation* and *personal_invitation* as instruments for *spillover_training* and *personal_training* and from two-stage estimations using *RPO_invitation_OL*, *RPO_invitation_OM*, *RPO_invitation_LM* and *personal_invitation* as instruments for *spillover_training_OL*, *spillover_training_OM*, *spillover_training_LM* and *personal_training*. Standard errors are reported in parentheses (clustered at RPO level).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Treatment effects at individual level—subsample of leaders

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta_{\text{negotiation}}$	$\Delta_{\text{integrity}}$	$\Delta_{\text{information}}$	$\Delta_{\text{efficiency}}$	Δ_{general}	Δ_{farming}	$\Delta_{\text{trust_sum}}$	$\Delta_{\text{trust_factor}}$
<i>Reduced form regressions</i>								
spillover_invitation	-0.1270 (0.1281)	-0.0159 (0.1091)	-0.0714 (0.1212)	-0.1071 (0.1101)	0.1706 (0.1099)	0.0992 (0.1262)	-0.0516 (0.5918)	-0.0260 (0.2516)
personal_invitation	-0.1498 (0.1291)	-0.0242 (0.1085)	-0.0507 (0.1287)	-0.0085 (0.1120)	0.2017 (0.1248)	0.1027 (0.1313)	0.0713 (0.6108)	0.0296 (0.2595)
<i>Instrumental variable regressions</i>								
spillover_training	-0.1318 (0.1421)	-0.0155 (0.1189)	-0.0804 (0.1322)	-0.1333 (0.1177)	0.1771 (0.1208)	0.1057 (0.1382)	-0.0783 (0.6382)	-0.0382 (0.2712)
personal_training	-0.1595 (0.1372)	-0.0264 (0.1141)	-0.0498 (0.1373)	0.0059 (0.1169)	0.2149 (0.1337)	0.1075 (0.1396)	0.0926 (0.6425)	0.0392 (0.2729)
N	168	168	168	168	168	168	168	168

Notes: We report coefficients from regressions on the subsample of leaders using both baseline and endline data regressing changes in trust on a constant, a binary indicator for personal treatment and a binary indicator for treatment of others. For instrumental variable regressions we report coefficients from two-stage estimations using $RPO_invitation$ and $personal_invitation$ as instruments for $spillover_training$ and $personal_training$. Standard errors are reported in parentheses (clustered at RPO level).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We run similar robustness checks on the full sample of 798 farmers as we did for the treatment effects on RPO level. First, we enlarge the model in first differences by allowing for onion-specific and rice-specific time trends and by controlling for the number of invited/trained persons per RPO. Qualitatively, results remain unchanged compared to the model in first differences without covariates. Second, we estimate ANCOVA specifications without covariates, with controls for onion and rice cultivation and with controlling for the number of invited/trained persons per RPO. Results are similar to those from the difference-in-differences estimations and robust to controlling for onion and rice cultivation and to controlling for the number of invitees/participants. For details on these robustness checks, see Appendix B.4.

Besides having an impact on intra-RPO trust in both members and leaders, the training intervention may have also affected farmers' perception of their RPO. Data about the true and perceived level of information is scarce in our data set. We have information on how well farmers know their own RPO, namely whether they believe to know how many members their RPO has. This question was only included in the endline survey. In Table 11 we report the percentage of farmers stating that they know the size of their RPO by intended treatment status. On average, farmers from invited RPOs feel significantly better informed about the size of their RPO than farmers in the control group. This difference is dominated by farmers who were personally invited, but also non-invited farmers in the treatment group fare better than farmers in the control group. Apparently, the training was able to provide farmers with information they were previously lacking. In line with this, we find that farmers in invited RPOs appear better informed about the potential benefits of an efficiently working RPO, and also a significant difference when looking at the evaluation of the actual RPOs (see Table 11). Farmers from invited RPOs are significantly more confident than farmers from the control group that their RPO can help to overcome hindrances that they encounter when selling individually.

Table 11: Perception of RPO at endline by intended treatment status

Panel A (N=798): Do you know how many members there are in your group (approximately)?					
treatment			control	total	p-value
52.93			33.83	48.12	0.0000
	invitees	non-invitees			
	74.67	45.64			0.0000
Panel B (N=532): Do you think an efficient group could overcome hindrances you encounter selling individually?					
treatment			control	total	p-value
97.96			95.68	97.37	0.1493
	invitees	non-invitees			
	95.61	98.92			0.0350
Panel C (N=533): Do you think your group could overcome hindrances you encounter selling individually?					
treatment			control	total	p-value
67.18			52.86	63.41	0.0025
	invitees	non-invitees			
	67.26	67.14			0.9827

Notes: We report the percentage of persons answering the questions affirmatively at endline as well as the p-value of two-sample t-tests with the null of equal means. Samples in panels B and C are smaller than the sample in panel A as people who stated to not be facing any constraints when selling individually did not answer these questions.

7 Concluding remarks

Although many positive examples from both developed and developing countries exist, many RPOs still struggle to offer the type of commercialization services that would lead to higher output prices for their members. In this paper, we have argued that trust in leaders' motives and competence as well as trust in members are crucial for successful collective commercialization as a form of collective action.

We conduct a cluster-randomized controlled trial in the context of RPOs in Senegal. We randomly invited members and/or leaders to participate in a three-day training to induce members and leaders to coordinate towards successful collective commercialization. The training itself consisted of three days of interactive discussion on the benefits, conduct and constraints of collective commercialization. In all treated RPOs, a general assembly was convened shortly after the training to discuss its content with non-participants.

We find a positive treatment effect of the training on both trust in leaders and trust in members. For trust in leaders, the effect is especially pronounced for trust in leaders' integrity and their informational advantage. Moreover, results suggest a strong spillover effect on non-treated members for trust in leaders, i.e., that the sizable positive average treatment effect is not to be attributed solely to personal invitation. This may be attributed to untrained members believing that

trained leaders have gained relevant skills (trust in competence) and that trained leaders are less likely to privately capture rents (trust in motives). These results are robust to the inclusion of different covariates and to different specifications of the underlying model.

These findings suggest that relatively soft and non-costly interventions such as a group training appear to be able to positively affect intra-RPO trust and the farmer's perception of coordination towards successful collective action. Furthermore, social capital is often thought to increase with use, i.e., it is thought of as being self-reinforcing when reciprocity connects people leading to increased trust and confidence (Pretty and Ward, 2001). Thus, the training may well have been the starting point for more collective endeavors to come. Bernard et al. (2014) show for a subsample of groundnut farmers that the training intervention indeed had a positive effect on the quantity of produce commercialized collectively through the RPOs. While being the motivation for this research, collective commercialization is only one dimension along which smallholders can benefit from the training intervention. Smallholders can benefit from the newly gained intra-RPO trust in many ways; RPOs also engage in various other joint activities aside from collective commercialization all of which require trust among members and leaders. For example, RPOs lend farming equipment or provide credit to their members. In case of the rice farmers, there is good case to believe that they benefit from increased trust when jointly managing irrigation systems. With well-developed social capital, local water-users' groups are able to use existing resources more efficiently than individuals working alone or in competition. The same holds true for forest management practiced by some RPOs (Pretty and Ward, 2001).

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Appendix

A Definition of variables

Variable	Wave	Description
<i>age</i>	baseline	age of the respondent in years
<i>bicycles</i>	endline	number of bicycles the respondent owns
<i>contribution</i>	endline	binary variable equal to one if the respondent has delivered agricultural produce for collective commercialization to the RPO in the agricultural season 2010/2011, zero otherwise
<i>distance to market</i>	endline	distance to nearest crop market in kilometers
<i>distance to storage</i>	baseline	distance to the RPO's storage facilities (meeting place, if there are no storage facilities) in walking minutes
<i>efficiency</i>	both	Binary variable equal to one if the respondent fully agrees to the following statement, zero otherwise: "If I invest my money or my produce in the RPO, it will be used efficiently"
<i>efficiency_ordinal</i>	both	trust in leaders' efficiency measured on a scale from 1 (lowest) to 4 (highest) based on the level of agreement to the following statement: "If I invest my money or my produce in the RPO, it will be used efficiently".
<i>farming</i>	both	binary variable equal to one if the respondent fully agrees to the following statement, zero otherwise: "I can trust the people from my RPO to look after my field during an absence of two months"
<i>farming_ordinal</i>	both	farming-specific trust in members measured on a scale from 1 (lowest) to 4 (highest) based on the level of agreement to the following statement: "I can trust the people from my RPO to look after my field during an absence of two months"
<i>general</i>	both	binary variable equal to one if the respondent fully agrees to the following statement, zero otherwise: "The majority of people from the RPO are trustworthy"
<i>general_ordinal</i>	both	general trust in members measured on a scale from 1 (lowest) to 4 (highest) based on the level of agreement to the following statement: "The majority of people from the RPO are trustworthy"
<i>groundnut</i>	both	binary variable equal to one if farmers organized in the RPO cultivate groundnuts, zero otherwise
<i>hectares</i>	baseline	size of managed land in hectares
<i>heterogeneity</i>	endline	binary variable equal to one if farmers organized in the RPO belong to more than one ethnicity, zero if they all belong to the same ethnicity
<i>household size information</i>	baseline	household size
	both	binary variable equal to one if the respondent fully agrees to the following statement, zero otherwise: "If the RPO's board says that I can achieve better prices if I wait a little, they do so because they have adequate information"

(...)

Variable	Wave	Description
<i>information_ordinal</i>	both	trust in leaders' informational advantage measured on a scale from 1 (lowest) to 4 (highest) based on the level of agreement to the following statement: "If the RPO's board says that I can achieve better prices if I wait a little, they do so because they have adequate information"
<i>integrity</i>	both	binary variable equal to one if the respondent fully agrees to the following statement, zero otherwise: "Board members defend the RPO's interests and their own interests in equal measure"
<i>integrity_ordinal</i>	both	trust in leaders' integrity measured on a scale from 1 (lowest) to 4 (highest) based on the level of agreement to the following statement: "Board members defend the RPO's interests and their own interests in equal measure"
<i>literate</i>	baseline	binary variable equal to one if the respondent is literate, zero otherwise
<i>male</i>	baseline	binary variable equal to one if the respondent is male, zero if she is female
<i>member</i>	baseline	binary variable equal to one if the respondent is a member, zero if (s)he is a leader
<i>members' initiative</i>	baseline	binary variable equal to one if the creation initiative came from group members, zero otherwise
<i>motorcycles</i>	endline	number of motorcycles the respondent owns
<i>negotiation</i>	both	binary variable equal to one if the respondent fully agrees to the following statement, zero otherwise: "The RPO's board is capable of negotiating better prices for our products than I am capable of myself"
<i>negotiation_ordinal</i>	both	trust in leaders' negotiation skills measured on a scale from 1 (lowest) to 4 (highest) based on the level of agreement to the following statement: "The RPO's board is capable of negotiating better prices for our products than I am capable of myself"
<i>onion</i>	both	binary variable equal to one if farmers organized in the RPO cultivate onions, zero otherwise
<i>personal_invitation</i>	both	binary variable equal to one if the respondent was invited, zero otherwise
<i>personal_training</i>	both	binary variable equal to one if the respondent participated in the training, zero otherwise
<i>phone</i>	endline	binary variable equal to one if any of the respondent's household's members owns a mobile phone, zero otherwise
<i>related to leader</i>	baseline	binary variable equal to one if the respondent is related to a leader
<i>rice</i>	both	binary variable equal to one if farmers organized in the RPO cultivate rice, zero otherwise
<i>RPO_invitation</i>	both	binary variable equal to one if at least one member or at least one leader of the RPO has been invited to participate in the training intervention, zero if nobody was invited

(...)

Variable	Wave	Description
<i>RPO_invitation_LM</i>	both	binary variable equal to one if at least one member and at least one leader of the RPO has been invited to participate in the training intervention, zero otherwise
<i>RPO_invitation_OL</i>	both	binary variable equal to one if at least one leader and no member of the RPO has been invited to participate in the training intervention, zero otherwise
<i>RPO_invitation_OM</i>	both	binary variable equal to one if at least one member and no leader of the RPO has been invited to participate in the training intervention, zero otherwise
<i>RPO_training</i>	both	binary variable equal to one if at least one member or at least one leader of the RPO participated in the training intervention, zero if nobody participated
<i>sales</i>	endline	binary variable equal to one if the RPO engaged in collective sales in the agricultural season 2010/2011, i.e., if at least one farmer sold produce to the RPO, zero if it did not
<i>savings in bank</i>	endline	binary variable equal to one if the respondent has a savings account with a bank, zero otherwise
<i>savings in MFI</i>	endline	binary variable equal to one if the respondent has a savings account with a société d'assurance, mutuelle, caisse or microfinance organization, zero otherwise
<i>size</i>	baseline	number of farmers that are part of the RPO
<i>spillover_invitation</i>	both	binary variable equal to one if somebody other than the respondent was invited, zero otherwise
<i>spillover_invitation_LM</i>	both	binary variable equal to one if somebody other than the respondent was invited under the “leaders and members” scheme, zero otherwise
<i>spillover_invitation_OL</i>	both	binary variable equal to one if some leader other than the respondent was invited under the “leaders only” scheme, zero otherwise
<i>spillover_invitation_OM</i>	both	binary variable equal to one if some member other than the respondent was invited under the “members only” scheme, zero otherwise
<i>spillover_training</i>	both	binary variable equal to one if somebody other than the respondent participated in the training, zero otherwise
<i>spillover_training_LM</i>	both	binary variable equal to one if somebody other than the respondent participated in the training under the “leaders and members” scheme, zero otherwise
<i>spillover_training_OL</i>	both	binary variable equal to one if some leader other than the respondent participated in the training under the “leaders only” scheme, zero otherwise
<i>spillover_training_OM</i>	both	binary variable equal to one if some member other than the respondent participated in the training under the “members only” scheme, zero otherwise
<i>trust_factor</i>	both	factor variable containing the six binary trust measures
<i>trust_sum</i>	both	sum over the six binary trust measures
<i>vehicles</i>	endline	number of motorized vehicles other than motorcycles the respondent owns

(...)

Variable	Wave	Description
<i>year of creation</i>	baseline	year in which the RPO was created
<i>year joined</i>	baseline	year in which the respondent joined the RPO
<i>number_invited</i>	both	number of people other than the respondent who were invited from a RPO
<i>number_invited_leaders</i>	both	number of leaders other than the respondent who were invited from a RPO
<i>number_invited_members</i>	both	number of members other than the respondent who were invited from a RPO
<i>number_trained</i>	both	number of people other than the respondent from a RPO who participated in the training
<i>number_trained_leaders</i>	both	number of leaders from a RPO who participated in the training
<i>number_trained_members</i>	both	number of members from a RPO who participated in the training

Notes: Data collection took place November/December 2011 (baseline) and January/February 2012 (endline).

B Supplementary analyses

In this appendix we provide additional analyses on selected issues discussed in the paper.

B.1 One-sided noncompliance

While invitations were distributed according to the randomization protocol, one-sided noncompliance regarding training participation was frequent. While none of the control RPOs participated in the training, some of the invited RPOs chose not to participate. From those RPOs that did participate, not always the exact farmers that had personally been invited to the training participated, sometimes others came to the training instead. In the empirical analysis, we deal with this by estimating intention-to-treat effects, i.e. the treatment effect of the invitation, and by estimating treatment effects of the training by instrumenting for the participation using the invitation as instrument. Tables B.1 and B.2 provide details on the one-sided noncompliance.

In Table B.1 we provide details based on the distribution of invitations while we consider actual participation in the training in Table B.2. In Table B.1 we report the intended distribution of 798 interviewed farmers from 73 RPOs on 50 invited and 23 non-invited RPOs, and the intended distribution of 597 interviewed farmers from 50 invited RPOs on 150 invitees and 447 non-invitees. Further, we report how the invitations were allocated to RPOs in which only leaders (OL), only members (OM), or both leaders and members (LM) were invited. We report both the number of farmers and the number of RPOs (in parentheses).

In Table B.2 we report the actual distribution of 798 interviewed farmers from 73 RPOs on 44 treated and 23 non-treated RPOs, and the distribution of 534 interviewed farmers from 44 treated RPOs on 142 trained and 392 non-trained farmers. Further, we report how the participation was allocated to RPOs in which only leaders (OL), only members (OM), or both leaders and members (LM) were trained. We report both the number of farmers and the number of RPOs (in parentheses).

Table B.1: Sample by intended treatment status

treatment							control	total
597							201	798
(50)							(23)	(73)
	invitees				non-invitees			
	150				447			
		OL	OM	LM		OL	OM	LM
		22	29	99		115	131	201
		(12)	(15)	(23)				

Notes: We report the intended distribution of 798 interviewed individuals from 73 RPOs on treatment and control group, and the intended distribution of 597 interviewed individuals from 50 treatment RPOs on invitees and non-invitees from different types of groups (OM—only members were invited, OL—only leaders were invited, LM—both members and leaders were invited). Values in parentheses are the number of RPOs.

Table B.2: Sample by actual treatment status

treatment							control	total
534							264	798
(44)							(29)	(73)
	trainees				non-trainees			
	142				392			
		OL	OM	LM		OL	OM	LM
		20	16	106		95	86	211
		(10)	(9)	(25)				

Notes: We report the actual distribution of 798 interviewed individuals from 73 RPOs on treatment and control group, and the distribution of 534 interviewed individuals from 44 treatment RPOs on trainees and non-trainees from different types of groups (OM—only members were trained, OL—only leaders were trained, LM—both members and leaders were trained). Values in parentheses are the number of RPOs.

B.2 Descriptive statistics for ordinal measures of trust

Originally, responses to all trust survey questions were recorded on a four-point Likert-type scale. The items to choose from are (1) “Do not agree at all”, (2) “Do not really agree”, (3) “Agree”, and (4) “Fully agree”. However, given that the average trust levels at baseline are well above 3 for all measures of trust, we use binary variables as regressands in the empirical analysis which are equal to one if respondents fully agree with one of the asked statements. Here, we report additional descriptive statistics for trust measures using the original coding.

In Table B.3 we show that the assignment of the invitations is also independent of ordinal trust measures at baseline. The two-sample Wilcoxon rank-sum tests with the null of equal means in control and treatment groups at baseline reveal no significant differences.

Table B.3: **Balancing tests of ordinal trust measures at RPO level**

Variable	Mean (N=73)	Standard deviation	Mean in control group (N=23)	Mean in treatment group (N=50)	Difference
negotiation_ordinal	3.41	0.37	3.34	3.44	-0.10
integrity_ordinal	3.51	0.42	3.51	3.50	0.01
information_ordinal	3.54	0.34	3.60	3.51	0.09
efficiency_ordinal	3.50	0.42	3.48	3.51	-0.03
general_ordinal	3.48	0.35	3.48	3.48	-0.01
farming_ordinal	3.43	0.37	3.36	3.46	-0.09

Notes: We report averages and standard deviations for the full sample of 73 RPOs, averages in the subsamples of RPOs in treatment and control group and results of two-sample Wilcoxon rank-sum tests with the null of equal means in control and treatment groups at baseline.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Descriptive statistics reveal a positive impact of the intervention on ordinal measures of trust. In Table B.4 we report results of two-sample Wilcoxon rank-sum tests with the null of equal means in ordinal trust in baseline and endline for invitees, non-invitees in invited RPOs and for non-invitees in non-invited RPOs. Moreover, we report two-sample Wilcoxon rank-sum tests with the null of equal means in ordinal trust for invitees and non-invitees in invited RPOs in both baseline and endline and for non-invitees in invited and non-invited RPOs in both baseline and endline. Results are similar to those when using binary trust measures (see Table 5). Invitees report higher levels of trust at endline compared to baseline for all ordinal measures for trust. Further indication of a positive di-

rect treatment effect comes from the comparison of invitees with non-invitees from invited RPOs. While we observe no significant difference at baseline, we observe significant differences for all ordinal measures of trust in leaders but *negotiation_ordinal* at endline. We also find descriptive evidence for a positive spill-over effect within invited RPOs. Comparing non-invitees from invited and non-invited RPOs, we find no significant differences at baseline. At endline, however, non-invitees in invited RPOs report significantly higher trust levels for all ordinal trust measures but *efficiency_ordinal* compared to non-invitees from non-invited RPOs.

Table B.4: Changes in ordinal trust by intended treatment status at RPO level

Ordinal Trust Outcomes	Sample	N	Baseline (BL)	Endline(EL)	Diff. BL EL P-value
negotiation_ordinal	A. Invitees	150	3.49	3.53	0.1960
	B. Non-Invitees in T	447	3.43	3.50	0.0029
	C. Non-Invitees in C	201	3.35	3.24	0.6423
	Diff. A B P-value		0.3362	0.7642	
	Diff. B C P-value		0.4157	0.0008	
integrity_ordinal	A. Invitees	150	3.59	3.59	0.4830
	B. Non-Invitees in T	447	3.47	3.39	0.5975
	C. Non-Invitees in C	201	3.49	3.21	0.0059
	Diff. A B P-value		0.1939	0.0236	
	Diff. B C P-value		0.7073	0.0168	
information_ordinal	A. Invitees	150	3.57	3.71	0.0310
	B. Non-Invitees in T	447	3.49	3.57	0.0499
	C. Non-Invitees in C	201	3.59	3.43	0.1928
	Diff. A B P-value		0.3902	0.0531	
	Diff. B C P-value		0.2187	0.0661	
efficiency_ordinal	A. Invitees	150	3.56	3.71	0.0698
	B. Non-Invitees in T	447	3.49	3.48	0.8127
	C. Non-Invitees in C	201	3.48	3.36	0.2672
	Diff. A B P-value		0.1990	0.0016	
	Diff. B C P-value		0.9008	0.1114	
general_ordinal	A. Invitees	150	3.51	3.69	0.0123
	B. Non-Invitees in T	447	3.46	3.52	0.2967
	C. Non-Invitees in C	201	3.46	3.31	0.1068
	Diff. A B P-value		0.5608	0.0053	
	Diff. B C P-value		0.8025	0.0029	
farming_ordinal	A. Invitees	150	3.47	3.63	0.0372
	B. Non-Invitees in T	447	3.45	3.45	0.5494
	C. Non-Invitees in C	201	3.36	3.22	0.1758
	Diff. AB P-value		0.6632	0.0166	
	Diff. BC P-value		0.3500	0.0036	

Notes: We report averages of different ordinal trust measures in subsamples of invitees, non-invitees in the treatment group (T—treatment group), non-invitees in the control group (C—control group) at baseline (BL—baseline) and endline (EL—endline) as well as p-values of two-sample Wilcoxon rank-sum tests with the null of equal means.

B.3 Robustness checks for treatment effects on RPO level

We run several robustness checks to confirm our main findings reported in Section 5.

First, we enlarge the model in first differences (see equations (1) and (4)) by allowing for onion-specific and rice-specific time trends (see Table B.5) and by controlling for the number of invited/trained persons per RPO (see Table B.6).³⁶ Table B.5 shows results for regressing changes in trust on a binary treatment indicator, a binary indicator for rice cultivation, a binary indicator for onion cultivation and a constant. We report both intention-to-treat effects as well as results from two-stage regressions using invitation status as an instrument for training participation. We use *RPO_invitation* as instrument for *RPO_training* in Table B.5. The highly significant correlation coefficient between these two variables amounts to 0.8354. Qualitatively, results remain unchanged compared to the model in first differences without covariates. We find a significantly positive treatment effect of the training of 12 to 13 percentage points on trust in leaders' integrity and their informational advantage. For the reduced form regressions, we provide conventional inference and randomization inference. We find a similar pattern as we did in our main analysis, p-values for the randomization inference are smaller than those for the conventional inference for all regressions. Hence, for reasons of clarity, we refrain from presenting randomization inference in the following.

Table B.6 shows results for regressing changes in trust on a binary treatment indicator, the number of invited farmers and a constant. We also run a specification where we consider the number of invited members and the number of invited leaders. Again, we report both intention-to-treat effects as well as results from two-stage regressions using invitation status as an instrument for training participation. We use *RPO_invitation* and *number_invited* as instruments for *RPO_trained* and *number_trained* (highly significant correlation coefficients range between 0.6467 and 0.9054) as well as *RPO_invitation*, *number_invited_members* and *number_invited_leaders* as instruments for *RPO_trained*, *number_trained_members* and *number_trained_leaders* in Table B.6 (highly significant correlation coefficients between 0.2458 and 0.9045). Qualitatively, results remain unchanged compared to our main results. The number of in-

³⁶Note that when including covariates to the model in first differences, it is no longer equivalent to a fixed effects model.

vited/trained persons has no additional effect on trust outcomes on top of the effect of the binary treatment measure. We find a significantly positive intention-to-treat effect of 14 to 16 percentage points on trust in leaders' integrity and their informational advantage.

Table B.5: Treatment effects at RPO level controlling for crop type

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ negotiation	Δ integrity	Δ information	Δ efficiency	Δ general	Δ farming	Δ trust_sum	Δ trust_factor
<i>Reduced form regressions (conventional and randomization inference)</i>								
RPO_invitation	0.0921 [×] (0.0623)	0.1109 ^{*, × ×} (0.0615)	0.1019 [×] (0.0619)	0.0329 (0.0688)	0.0819 (0.0586)	0.0775 (0.0632)	0.4972 [×] (0.3217)	0.2105 [×] (0.1372)
[p-value conventional inference]	[0.1440]	[0.0756]	[0.1043]	[0.6340]	[0.1668]	[0.2239]	[0.1266]	[0.1294]
{p-value randomization inference}	{0.0947}	{0.0360}	{0.0664}	{0.5499}	{0.1213}	{0.1438}	{0.0742}	{0.0767}
<i>Instrumental variable regressions (conventional inference)</i>								
RPO_training	0.1082 (0.0725)	0.1303 [*] (0.0716)	0.1197 [*] (0.0719)	0.0386 (0.0789)	0.0962 (0.0670)	0.0911 (0.0726)	0.5841 (0.3725)	0.2473 (0.1589)
[p-value conventional inference]	[0.1355]	[0.0687]	[0.0959]	[0.6242]	[0.1512]	[0.2098]	[0.1169]	[0.1196]
N	73	73	73	73	73	73	73	73

Notes: We report coefficients from regressions using both baseline and endline data regressing changes in trust on a constant, a binary treatment indicator and binary indicators for onion and rice cultivation. For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation* as instrument for *RPO_training*. Standard errors are reported in parentheses (clustered at RPO level), p-values for conventional inference are reported in brackets, p-values for randomization inference with 9000 random permutations are reported in curly brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, for conventional inference

× $p < 0.10$, × × $p < 0.05$, × × × $p < 0.01$, for randomization inference

Table B.6: Treatment effects at RPO level controlling for treatment intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ negotiation							
	Δ integrity							
	Δ information							
	Δ trust_factor							
<i>Reduced form regressions</i>								
RPO_invitation	0.1182 (0.0767)	0.1185 (0.0768)	0.1560** (0.0757)	0.1560** (0.0763)	0.1428* (0.0826)	0.1431* (0.0827)	0.0387 (0.0840)	0.0389 (0.0845)
number_invited	-0.0022 (0.0062)		-0.0051 (0.0052)		-0.0037 (0.0069)		0.0036 (0.0051)	
number_invited_members		0.0020 (0.0074)		-0.0058 (0.0069)		0.0017 (0.0083)		0.0074 (0.0055)
number_invited_leaders		-0.0072 (0.0090)		-0.0042 (0.0080)		-0.0100 (0.0100)		-0.0100 (0.0089)
<i>Instrumental variable regressions</i>								
RPO_training	0.1713 (0.1153)	0.1765 (0.1152)	0.2274** (0.1145)	0.2274** (0.1150)	0.2075 (0.1267)	0.2140* (0.1267)	0.0532 (0.1206)	0.0578 (0.1214)
number_trained	-0.0080 (0.0102)		-0.0131 (0.0095)		-0.0109 (0.0120)		0.0023 (0.0085)	
number_trained_members		-0.0025 (0.0107)		-0.0131 (0.0111)		-0.0040 (0.0123)		0.0072 (0.0079)
number_trained_leaders		-0.0160 (0.0139)		-0.0131 (0.0123)		-0.0208 (0.0158)		-0.0046 (0.0132)
<i>Reduced form regressions</i>								
RPO_invitation	0.0740 (0.0766)	0.0739 (0.0772)	0.1225 (0.0790)	0.1224 (0.0795)	0.6523 (0.4069)	0.6527 (0.4096)	0.2769 (0.1737)	0.2771 (0.1749)
number_invited	0.0051 (0.0075)		-0.0035 (0.0060)		-0.0057 (0.0283)		-0.0024 (0.0121)	
number_invited_members		0.0023 (0.0090)		-0.0062 (0.0071)		0.0014 (0.0330)		0.0007 (0.0141)
number_invited_leaders		0.0085 (0.0112)		-0.0002 (0.0093)		-0.0141 (0.0448)		-0.0062 (0.0191)
<i>Instrumental variable regressions</i>								
RPO_training	0.1030 (0.1117)	0.1007 (0.1125)	0.1783 (0.1117)	0.1760 (0.1159)	0.9407 (0.6046)	0.9525 (0.6092)	0.3994 (0.2581)	0.4045 (0.2601)
number_trained	0.0025 (0.0114)		-0.0097 (0.0095)		-0.0369 (0.0492)		-0.0157 (0.0210)	
number_trained_members		0.0001 (0.0120)		-0.0122 (0.0097)		-0.0244 (0.0503)		-0.0102 (0.0214)
number_trained_leaders		0.0059 (0.0155)		-0.0062 (0.0135)		-0.0547 (0.0685)		-0.0235 (0.0292)
N	73	73	73	73	73	73	73	73

Notes: We report coefficients from regressions using both baseline and endline data regressing changes in trust on a constant, a binary treatment indicator and controls for the number of farmers (members and leaders) invited. For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation* and *number_invited* as instruments for *RPO_training* and *number_trained*. In even-numbered columns we report coefficients from two-stage estimations using *RPO_invitation*, *number_invited_members* and *number_invited_leaders* as instruments for *RPO_training*, *number_trained_members* and *number_trained_leaders*. Standard errors are reported in parentheses (clustered at RPO level).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Second, we follow McKenzie (2012) who argues that large improvements in power can arise when estimating treatment effects via an Analysis of Covariance (ANCOVA) estimation compared to using the difference-in-differences specification. While difference-in-differences fully corrects for potential baseline imbalances between treatment and control groups, which may be inefficient if baseline data have little predictive power for future outcomes, ANCOVA reduces the variance of the treatment effect estimator by conditioning on a variable that is correlated with the outcome of interest. ANCOVA includes the mean value of the outcome over all available pre-treatment rounds as a covariate when estimating the treatment effect. In our case of one pre-treatment and one post-treatment survey, it includes the baseline value of the outcome as a covariate.

Thus, we regress trust at endline on a constant, a binary treatment indicator and on trust at baseline. We estimate these regressions without covariates, with controls for onion and rice cultivation (see Table B.7) and with control for the number of invited/trained persons per RPO (see Table B.8).

We use *RPO_invitation* as instrument for *RPO_training* in Table B.7 and *RPO_invitation* and *number_invited* as instruments for *RPO_trained* and *number_trained* in Table B.8. Results are similar to those from the difference-in-differences estimations. Without covariates, we find a significantly positive intention-to-treat effect of 10 to 12 percentage points for trust in leaders' negotiation skills, their integrity, and their informational advantage. We find a (significantly) positive intention-to-treat effect of 11 percentage points on both measures of trust in members. The treatment effect of the training itself is again slightly larger. These results are robust to controlling for onion and rice cultivation and to controlling for the number of invitees/participants.

Table B.7: Treatment effects at RPO level—ANCOVA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: no covariates	negotiation	integrity	information	efficiency	general	farming	trust_sum	trust_factor
<i>Reduced form regressions</i>								
RPO_invitation	0.1210** (0.0549)	0.1228** (0.0599)	0.1034* (0.0558)	0.0666 (0.0659)	0.1096* (0.0592)	0.1074 (0.0658)	0.6281* (0.3318)	0.2660* (0.1415)
trust outcome at baseline	0.4369*** (0.1161)	0.7222*** (0.1099)	0.6014*** (0.1299)	0.6380*** (0.1266)	0.7705*** (0.0926)	0.8141*** (0.1419)	0.7736*** (0.1150)	0.7735*** (0.1153)
<i>Instrumental variable regressions</i>								
RPO_training	0.1392** (0.0616)	0.1390** (0.0658)	0.1152* (0.0601)	0.0762 (0.0731)	0.1257* (0.0656)	0.1239* (0.0736)	0.7165* (0.3664)	0.3033* (0.1562)
trust outcome at baseline	0.3815*** (0.1315)	0.6721*** (0.1127)	0.5558*** (0.1330)	0.6113*** (0.1308)	0.7254*** (0.0972)	0.7599*** (0.1481)	0.7178*** (0.1241)	0.7183*** (0.1242)
Panel B: controls for crop type								
<i>Reduced form regressions</i>								
RPO_invitation	0.0940* (0.0494)	0.0840* (0.0488)	0.0490 (0.0450)	0.0234 (0.0485)	0.0681 (0.0498)	0.0690 (0.0530)	0.3966 (0.2540)	0.1665 (0.1080)
trust outcome at baseline	0.2826** (0.1098)	0.4694*** (0.1250)	0.3130** (0.1313)	0.3327** (0.1326)	0.4148*** (0.0998)	0.3609** (0.1658)	0.4283*** (0.1308)	0.4278*** (0.1310)
<i>Instrumental variable regressions</i>								
RPO_training	0.1106* (0.0567)	0.0973* (0.0546)	0.0562 (0.0497)	0.0274 (0.0547)	0.0795 (0.0555)	0.0807 (0.0595)	0.4602 (0.2834)	0.1931 (0.1205)
trust outcome at baseline	0.2493** (0.1142)	0.4452*** (0.1234)	0.2974** (0.1267)	0.3255** (0.1304)	0.3944*** (0.0967)	0.3339** (0.1595)	0.4004*** (0.1302)	0.4005*** (0.1301)
N	73	73	73	73	73	73	73	73

Notes: We report coefficients from regressing trust at endline on a constant and a binary treatment indicator including the trust level at baseline as covariate. In panel B we report coefficients from regressions additionally including binary indicators for onion and rice cultivation. For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation* as instrument for *RPO_training*. Standard errors are reported in parentheses (clustered at RPO level).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: Treatment effects at RPO level controlling for treatment intensity—ANCOVA

Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	negotiation	integrity	information	efficiency	general	farming	trust_sum	trust_factor
<i>Reduced form regressions</i>								
RPO_invitation	0.1385** (0.0656)	0.1435** (0.0703)	0.1188 (0.0726)	0.0398 (0.0766)	0.0833 (0.0714)	0.1216 (0.0746)	0.6362 (0.3864)	0.2693 (0.1649)
number_invited	-0.0033 (0.0070)	-0.0039 (0.0061)	-0.0029 (0.0076)	0.0051 (0.0064)	0.0050 (0.0075)	-0.0027 (0.0063)	-0.0015 (0.0325)	-0.0006 (0.0139)
trust outcome at baseline	0.4342*** (0.1152)	0.7235*** (0.1095)	0.6008*** (0.1304)	0.6359*** (0.1284)	0.7712*** (0.0945)	0.8159*** (0.1425)	0.7736*** (0.1157)	0.7735*** (0.1160)
<i>Instrumental variable regressions</i>								
RPO_training	0.1998** (0.0956)	0.2007** (0.0994)	0.1643 (0.1026)	0.0514 (0.1052)	0.1135 (0.0986)	0.1720* (0.1022)	0.8876 (0.5405)	0.3754 (0.2306)
number_trained	-0.0105 (0.0108)	-0.0108 (0.0100)	-0.0087 (0.0118)	0.0043 (0.0093)	0.0021 (0.0107)	-0.0084 (0.0093)	-0.0298 (0.0514)	-0.0126 (0.0219)
trust outcome at baseline	0.3669*** (0.1318)	0.6677*** (0.1119)	0.5449*** (0.1364)	0.6118*** (0.1326)	0.7270*** (0.0976)	0.7577*** (0.1446)	0.7137*** (0.1243)	0.7143*** (0.1243)
N	73	73	73	73	73	73	73	73

Notes: We report coefficients from regressing trust at endline on a constant and a binary treatment indicator including the trust level at baseline and the number of invited persons per RPO as covariates. For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation* and *number_invited* as instrument for *RPO_training* and *number_trained*. Standard errors are reported in parentheses (clustered at RPO level).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.4 Robustness checks for treatment effects on individual level

To confirm our main findings on treatment effects on individual level presented in Section 6, we run similar robustness checks as we did for the treatment effects on RPO level.

First, using the full sample of 798 farmers, we enlarge the model in first differences (see equations (5) and (6)) by allowing for onion-specific and rice-specific time trends (see Table B.9) and by controlling for the number of invited/trained persons per RPO (see Table B.10).³⁷ Table B.9 shows results for regressing changes in trust on a binary treatment indicator for personal treatment, a binary indicator for treatment of others, a binary indicator for rice cultivation, a binary indicator for onion cultivation and a constant. We report both intention-to-treat effects as well as results from two-stage regressions using invitation status as an instrument for training participation. We instrument for *spillover_training* and *personal_training* using *RPO_invitation* and *personal_invitation* as instruments.³⁸ Qualitatively, results remain unchanged compared to the model in first differences without covariates. We find a significantly positive effect of having personally been invited of 13 to 18 percentage points on trust in leaders' integrity and their informational advantages as well as on both measures of trust in members. The direct treatment effect of the training is again slightly larger.

Table B.10 shows results for regressing changes in trust on a binary treatment indicator for personal treatment, a binary indicator for treatment of others, the number of invited farmers and a constant. We report both intention-to-treat effects as well as results from two-stage regressions using invitation status as an instrument for training participation. We use *RPO_invitation*, *number_invited* and *personal_invitation* as instruments for *spillover_training*, *number_trained* and *personal_training*. The highly significant correlation coefficients range between 0.2700 and 0.9004. Qualitatively, results remain unchanged compared to the model in first differences without covariates. We find a significantly positive effect of having personally been invited of 19 to 22 percentage points on trust in leaders' integrity and their informational advantages as well as on both measures

³⁷Note that when including covariates to the model in first differences, it is no longer equivalent to a fixed effects model.

³⁸The highly significant correlation coefficients are 0.5702 for *spillover_training* and *RPO_invitation*, 0.2700 for *personal_training* and *RPO_invitation*, -0.3509 for *personal_invitation* and *spillover_training* and 0.7993 for *personal_invitation* and *personal_training*.

of trust in members. The direct treatment effect of the training is again slightly larger.

Second, we estimate ANCOVA specifications. We regress trust at endline on a constant, a binary treatment indicator for personal treatment, a binary indicator for treatment of others, and on trust at baseline. We estimate these regressions without covariates, with controls for onion and rice cultivation (see Table B.11) and with controlling for the number of invited/trained persons per RPO (see Table B.12). In the regressions reported in Table B.11 we use *RPO_invitation* and *personal_invitation* as instruments for *spillover_training* and *personal_training*. For table B.12 we use *RPO_invitation*, *number_invited* and *personal_invitation* as instruments for *spillover_training*, *number_trained* and *personal_training*. Results are similar to those from the difference-in-differences estimations. Without covariates, we find a significantly positive treatment effect of having personally been invited of 13 to 23 percentage points for all measures of trust and indication for a spillover effect of 11 to 13 percentage points for trust in leaders' negotiation skills and their integrity. The treatment effect of the training itself is again slightly larger. Results are again robust to controlling for onion and rice cultivation and to controlling for the number of invitees/participants.

Table B.9: Treatment effects on individual level controlling for crop type

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ negotiation	Δ integrity	Δ information	Δ efficiency	Δ general	Δ farming	Δ trust_sum	Δ trust_factor
<i>Reduced form regressions</i>								
spillover_invitation	0.0930 (0.0657)	0.1045 (0.0657)	0.0842 (0.0662)	0.0324 (0.0727)	0.0662 (0.0618)	0.0565 (0.0673)	0.4368 (0.3496)	0.1849 (0.1492)
personal_invitation	0.0617 (0.0716)	0.1535** (0.0679)	0.1323* (0.0703)	0.1050 (0.0759)	0.1794** (0.0747)	0.1427* (0.0765)	0.7746** (0.3611)	0.3306** (0.1539)
<i>Instrumental variable regressions</i>								
spillover_training	0.1124 (0.0796)	0.1210 (0.0793)	0.0715 (0.0798)	0.0338 (0.0864)	0.0970 (0.0727)	0.0616 (0.0796)	0.4973 (0.4184)	0.2103 (0.1785)
personal_training	0.0623 (0.0821)	0.1748** (0.0761)	0.1515* (0.0798)	0.1256 (0.0827)	0.2129** (0.0862)	0.1668* (0.0865)	0.8959** (0.4025)	0.3826** (0.1715)
N	798	798	798	798	798	798	798	798

Notes: The number of invited/trained persons in treated RPOs ranges from 2 to 16. Hence, it is impossible to be the only invited/treated person in a treated RPO. We report coefficients from regressions using both baseline and endline data regressing changes in trust on a constant, a binary treatment indicator for personal treatment and for treatment of others and binary indicators for onion and rice cultivation. For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation* and *personal_invitation* as instruments for *spillover_training* and *personal_training*. Standard errors are reported in parentheses (clustered at RPO level).

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: Treatment effects on individual level controlling for treatment intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta_{\text{negotiation}}$	$\Delta_{\text{integrity}}$	$\Delta_{\text{information}}$	$\Delta_{\text{efficiency}}$	Δ_{general}	Δ_{farming}	$\Delta_{\text{trust_sum}}$	$\Delta_{\text{trust_factor}}$
<i>Reduced form regressions</i>								
spillover_invitation	0.1132 (0.0761)	0.1647** (0.0774)	0.1369 (0.0836)	0.0549 (0.0866)	0.0787 (0.0817)	0.1164 (0.0825)	0.6647 (0.4254)	0.2828 (0.1817)
personal_invitation	0.0795 (0.0886)	0.2224*** (0.0807)	0.1910** (0.0858)	0.1247 (0.0847)	0.1892** (0.0842)	0.2097** (0.0903)	1.0166** (0.4273)	0.4348** (0.1821)
number_invited	-0.0004 (0.0059)	-0.0072 (0.0050)	-0.0046 (0.0064)	0.0009 (0.0048)	0.0022 (0.0077)	-0.0056 (0.0060)	-0.0148 (0.0264)	-0.0064 (0.0113)
<i>Instrumental variable regressions</i>								
spillover_training	0.1613 (0.1136)	0.2364** (0.1163)	0.1949 (0.1270)	0.0731 (0.1242)	0.1035 (0.1204)	0.1651 (0.1191)	0.9344 (0.6259)	0.3976 (0.2673)
personal_training	0.1118 (0.1216)	0.3048*** (0.1137)	0.2596** (0.1221)	0.1618 (0.1173)	0.2441** (0.1174)	0.2823** (0.1241)	1.3644** (0.5993)	0.5835** (0.2555)
number_trained	-0.0052 (0.0095)	-0.0159* (0.0090)	-0.0116 (0.0111)	-0.0014 (0.0082)	-0.0010 (0.0120)	-0.0120 (0.0096)	-0.0471 (0.0467)	-0.0202 (0.0200)
N	798	798	798	798	798	798	798	798

Notes: The number of invited/trained persons in treated RPOs ranges from 2 to 16. Hence, it is impossible to be the only invited/treated person in a treated RPO. We report coefficients from regressions using both baseline and endline data regressing changes in trust on a constant, a binary treatment indicator for personal treatments and for treatment of others and on the number of invited/trained persons. For instrumental variable regressions we report coefficients from two-stage estimations using $RPO_invitation$, $number_invited$ and $personal_invitation$ as instruments for $spillover_training$, $number_trained$ and $personal_training$. Standard errors are reported in parentheses (clustered at RPO level).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.1.1: Treatment effects on individual level—ANCOVA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: no covariates	negotiation	integrity	information	efficiency	general	farming	trust_sum	trust_factor
<i>Reduced form regressions</i>								
spillover_invitation	0.1280** (0.0539)	0.1107* (0.0611)	0.0787 (0.0594)	0.0609 (0.0651)	0.1029 (0.0625)	0.1007 (0.0662)	0.5812* (0.3389)	0.2450* (0.1446)
personal_invitation	0.1319** (0.0637)	0.1893*** (0.0680)	0.1476** (0.0644)	0.1763*** (0.0663)	0.2265*** (0.0711)	0.1998*** (0.0716)	1.0487*** (0.3645)	0.4464*** (0.1552)
trust outcome at baseline	0.1897*** (0.0375)	0.3599*** (0.0397)	0.2893*** (0.0406)	0.3111*** (0.0489)	0.3380*** (0.0436)	0.3744*** (0.0417)	0.3997*** (0.0430)	0.4008*** (0.0431)
<i>Instrumental variable regressions</i>								
spillover_training	0.1461** (0.0613)	0.1209* (0.0694)	0.0848 (0.0675)	0.0619 (0.0740)	0.1096 (0.0704)	0.1088 (0.0747)	0.6326* (0.3832)	0.2663 (0.1635)
personal_training	0.1402** (0.0712)	0.2144*** (0.0744)	0.1681** (0.0703)	0.2079*** (0.0709)	0.2620*** (0.0777)	0.2294*** (0.0773)	1.1950*** (0.3944)	0.5091*** (0.1679)
trust outcome at baseline	0.1801*** (0.0380)	0.3476*** (0.0411)	0.2798*** (0.0421)	0.3011*** (0.0503)	0.3241*** (0.0455)	0.3627*** (0.0429)	0.3846*** (0.0452)	0.3857*** (0.0453)
Panel B: controls for crop type								
<i>Reduced form regressions</i>								
spillover_invitation	0.0844* (0.0502)	0.0553 (0.0527)	0.0201 (0.0496)	-0.0005 (0.0515)	0.0350 (0.0520)	0.0362 (0.0542)	0.2460 (0.2752)	0.1020 (0.1173)
personal_invitation	0.0923* (0.0549)	0.1380** (0.0563)	0.0909* (0.0512)	0.1202** (0.0516)	0.1613*** (0.0576)	0.1380** (0.0580)	0.7401** (0.2816)	0.3148** (0.1199)
trust outcome at baseline	0.1564*** (0.0386)	0.2825*** (0.0396)	0.2282*** (0.0398)	0.2297*** (0.0449)	0.2403*** (0.0360)	0.2821*** (0.0390)	0.3035*** (0.0421)	0.3043*** (0.0422)
<i>Instrumental variable regressions</i>								
spillover_training	0.0996* (0.0600)	0.0600 (0.0625)	0.0190 (0.0590)	-0.0085 (0.0611)	0.0332 (0.0609)	0.0363 (0.0637)	0.2582 (0.3250)	0.1067 (0.1385)
personal_training	0.1019 (0.0630)	0.1631** (0.0630)	0.1098* (0.0574)	0.1497*** (0.0571)	0.1951*** (0.0644)	0.1661** (0.0646)	0.8825*** (0.3125)	0.3757*** (0.1330)
trust outcome at baseline	0.1520*** (0.0379)	0.2776*** (0.0396)	0.2239*** (0.0397)	0.2249*** (0.0447)	0.2334*** (0.0363)	0.2775*** (0.0386)	0.2969*** (0.0419)	0.2977*** (0.0420)
N	798	798	798	798	798	798	798	798

Notes: The number of invited/trained persons in treated RPOs ranges from 2 to 16. Hence, it is impossible to be the only invited/treated person in a treated RPO. We report coefficients from regressions regressing trust at endline on a constant, a binary treatment indicator for personal treatment and for treatment of others including the trust level at baseline as covariate. In panel B we report coefficients from regressions additionally including binary indicators for onion and rice cultivation. For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation* and *personal_invitation* as instruments for *spillover_training* and *personal_training*. Standard errors are reported in parentheses (clustered at RPO level).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.12: Treatment effects on individual level controlling for treatment intensity—ANCOVA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	negotiation	integrity	information	efficiency	general	farming	trust_sum	trust_factor
<i>Reduced form regressions</i>								
spillover_invitation	0.1656** (0.0675)	0.1550* (0.0780)	0.1198 (0.0806)	0.0546 (0.0818)	0.1038 (0.0817)	0.1307 (0.0800)	0.7163* (0.4234)	0.3024* (0.1808)
personal_invitation	0.1773** (0.0714)	0.2429*** (0.0719)	0.1973*** (0.0742)	0.1686** (0.0740)	0.2359*** (0.0749)	0.2359*** (0.0803)	1.2119*** (0.3885)	0.5157*** (0.1656)
number_invited	-0.0064 (0.0078)	-0.0075 (0.0080)	-0.0070 (0.0084)	0.0011 (0.0081)	-0.0002 (0.0092)	-0.0051 (0.0084)	-0.0230 (0.0428)	-0.0097 (0.0182)
trust outcome at baseline	0.1877*** (0.0377)	0.3597*** (0.0382)	0.2882*** (0.0401)	0.3111*** (0.0491)	0.3380*** (0.0433)	0.3746*** (0.0415)	0.3991*** (0.0424)	0.4002*** (0.0426)
<i>Instrumental variable regressions</i>								
spillover_training	0.2403** (0.1013)	0.2213* (0.1157)	0.1716 (0.1203)	0.0705 (0.1176)	0.1415 (0.1162)	0.1846 (0.1138)	1.0108 (0.6158)	0.4265 (0.2630)
personal_training	0.2493** (0.1001)	0.3307*** (0.1021)	0.2687** (0.1059)	0.2179** (0.1038)	0.2990*** (0.1022)	0.3170*** (0.1109)	1.633*** (0.5460)	0.6947*** (0.2329)
number_trained	-0.0149 (0.0118)	-0.0160 (0.0125)	-0.0138 (0.0132)	-0.0014 (0.0118)	-0.0051 (0.0130)	-0.0120 (0.0122)	-0.0600 (0.0646)	-0.0254 (0.0275)
trust outcome at baseline	0.1741*** (0.0382)	0.3436*** (0.0383)	0.2798** (0.0421)	0.3008*** (0.0499)	0.3222*** (0.0455)	0.3604*** (0.0419)	0.3803*** (0.0436)	0.3815*** (0.0437)
N	798	798	798	798	798	798	798	798

Notes: The number of invited/trained persons in treated RPOs ranges from 2 to 16. Hence, it is impossible to be the only invited/trained person in a treated RPO. We report coefficients from regressions regressing trust at endline on a constant, a binary treatment indicator for personal treatment and for treatment of others including the trust level at baseline and the number of invited/trained persons as covariate. For instrumental variable regressions we report coefficients from two-stage estimations using *RPO_invitation*, *number_invited* and *personal_invitation* as instruments for *spillover_training*, *number_trained* and *personal_training*. Standard errors are reported in parentheses (clustered at RPO level).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$